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# A STUDY IN THE FORMATION OF OPINION BASED UPON LEGAL EVIDENCE

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The members of the jury, the judge, and the advocates in the trial of a case have an opinion to form; it consists in deciding from the evidence presented whether or not the defendent is guilty as charged. The evidence itself is presented serially, one witness following another. Some parts of the testimony are more significant for the final result than others. Furthermore some witnesses are more reliable, some more persuasive, some more impressive under cross-examination than others. Presumably each bit of evidence must, in effect, be sifted, weighed and evaluated as for or against the plea of the defendent. In the end, if the evidence is conclusive, an opinion is formed with respect to the guilt of the defendent. No one seems to know, however, how the organism functions psychologically in solving a problem of this kind. Utterback, it is true, had made a useful provisional analysis of the psychological functions involved in the forming of various opinions; but there are no experiments.[1] Michael and Alder have set forth a purely logical analysis of judicial proof that is so far removed from psychology as to be of no service to it.[2] Wigmore, the only jurist who has faced the problem with some understanding of its psychological implications, has, in the absence of any aid from psychology been forced to a logical analysis.[3] From what is known of the psychological functions of comprehension and thinking it is highly probable that the operation

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[1] W. E. Utterback, Opining, This JOURNAL, 46, 1934, 503-506.

[2] J. Michael and M. J. Adler, *The Nature of Judicial Proof: An Inquiry into the Logical, Legal and Emperical Aspects of the Law of Evidence* (Columbia University, privately printed), 1931, 1-388.

[3] J. H. Wigmore, *The Principles of Judicial Proof*, 1913, 1-1179; 1931, 1-1056; *The Science of Judicial Proof*, 1937, 1-1065.



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is not as simple as a logical analysis would lead us to expect. In this experiment we hope to throw some light upon the question.

In our search for a method we sought a device by means of which the course of the solution could be gauged at various stages. Whether such a procedure would actually conform to that of a juror we cannot say. Consequently we do not claim to test the ordinary court procedure. Not only have we, in the experiment which we shall describe, stopped the flow of evidence at stated points and obtained judgments at these points, but we have also eliminated the summing up of the evidence by the trial lawyers, and the discussion of the evidence in the jury-room.[4] Furthermore the evidence which we employed was only the report of a case, and therefore our 'jurors' (*Ss*) had no opportunity of seeing the witnesses or of hearing the testimony exactly as it was presented. Our results should, however, throw some light upon the formation of opinions at large, and in so far as they do, they should consequently afford some insight into the way in which a juror functions psychologically.

*Method.* We selected as material the report of the Thomas Hoag Case, a celebrated criminal trial in which the defendent was charged with bigamy. The testimony as reported could be presented in one hour. If given in the order in which it is reported its nature is such that anything like certainty can hardly be reached before the end. The testimony was read slowly and distinctly to groups of college upperclassmen who were preparing for the law. The same group was never used more than once and consequently the experiment has continued through several years. Before the reading began, the important data—the name and alias of the defendent, the important dates, the names of his reputed wives, and the place-names were written on a blackboard. The testimony was divided into installments (hereinafter designated by Roman numerals) and at the end of each section the reader stopped and asked every member of the group to record his judgment at that point of the relative guilt or innocence of the defendent on a 9-point scale. This scale was as follows: 1, certainty of innocence; 2, strong belief in innocence; 3, fair belief in innocence; 4, slight belief in innocence; 5, doubtful; 6, slight belief in guilt; 7, fair belief in guilt; 8, strong belief in gult; 9, certanty of guilt.

The first installment (I) was the report of the indictment. This of course was not testimony; we simply desired to see whether or not the indictment had any effect on the determination of the *Ss*. The last installment (XIII) was the report

[4] Experiments have been made to determine the effect of the discussion of a body of evidence on a jury. The results are quantitative and show in general that judgments made before discussion are by many individuals changed in one direction or another after discussion; they tell us little or nothing about the reasons of the individuals for the change in opinion. See H. E. Burtt, Sex differences in the effect of discussion, *Jour. Exper. Psychol.*, 3, 1920, 390-395; W. M. Marston, Studies in testimony, *J. Crim. Law & Criminol.*, 15, 1924, 5-31; J. F. Dashiell, Experiments in the sifting of testimony, *Psychol. Bull.*, 30, 1933, 720; Experimental studies of the influence of social situations on the behavior of individual human adults, *Handbook of Social Psychology*, Clark Univ. Press, 1935, 1097-1158, esp. 1135 ff.

of the verdict when the case was tried. This was given to see what effect, if any, the finding of the jury had upon the final judgment of our *Ss*.

After all the installments had been read and the judgments recorded, every *S* was asked to write a brief statement about the evidence that was most influential for the determination of his judgments.

The only variation in procedure from year to year was in the order in which the installments were presented. The evidence as divided into installments was as follows.[5]

*I. Indictment.* The prisoner was indicted for that whereas Thomas Hoag, late of Haverstraw, in the county of Rockland, laborer, otherwise called Joseph Parker, now of the city of New York, cartman, on the 8th of May, 1797, at the said city of New York, was lawfully married to Susan Faesch, and the said Susan then and there had for a wife,[6] and the said Thomas, alias, etc., afterwards, to wit, on the 25th day of December, 1800, at the county of Rockland, his said wife being then in full life, feloniously did marry, and to wife did take, one Catherine Secor, etc. To this, the prisoner pleaded not guilty. *Judgment.*

*II. Prosecution.* On the part of the prosecution, Benjamin Coe testified: That he was one of the judges of the court of common pleas in the county of Rockland; that he well knew the prisoner at the bar; that he came to Rockland in the beginning of September, in the year 1800, and there passed by the name of Thomas Hoag; that there was a person with him who passed for his brother; but between those two persons there was no sort of resemblance; that the prisoner worked for witness about a month, during which time he ate daily at witness' table, and he of course saw him daily; that on the 25th day of December, 1800, witness married the prisoner to one Catherine Secor; that witness is confident of the time, because he recollected that on that very day one of his own children was christened; that during all the time the prisoner remained in Rockland county witness saw him continually; he was therefore as much satisfied that the prisoner was Thomas Hoag as that he himself was Benjamin Coe. *Judgment.*

*III. Prosecution.* John Knapp testified, that he knew the prisoner in 1800 and 1801; he was then in Rockland county, and passed by the name of Thomas Hoag; that he saw him constantly for five months, during the time the prisoner was at Rockland; that he was at the prisoner's wedding; that Hoag had a scar under his foot; the way that witness knew it, was that he and Hoag were leaping together, and witness outleaped Hoag, upon which the latter remarked that he could not leap as well now as formerly, in consequence of a wound in his foot by treading on a drawing knife; that Hoag then pulled off his shoe and showed witness the scar under his foot, occasioned by that wound; the scar was very perceptible. Witness was confident prisoner at the bar was Thomas Hoag.

Catherine Conklin (formerly Catherine Secor) testified, that she became acquainted with prisoner in the beginning of September, 1800, when he came to Rockland; he then passed by the name of Thomas Hoag; that witness saw him constantly; that prisoner, shortly after their acquaintance, paid his addresses to her,

[5] This report of the Thomas Hoag trial is taken from Wigmore, *op. cit.*; 1913, 714-720; 1931, 713-719; 1937, 888-896. The case was tried in the Court of Oyer and Terminer, New York City, June 22, 1804.

[6] The defense did not contest this marriage.

and finally, on the 25th of December, married her; that he lived with her till the latter end of March, 1801, when he left her; that she did not see him again until two years after; that on the morning of his leaving her, he appeared desirous of communicating something to her of importance but was dissuaded from it by a person who was with him and who passed for his brother; that Hoag, until his departure, was a kind, attentive and affectionate husband; that she was as well convinced as she could possibly be of anything in this world, that the prisoner at the bar was the person who married her by the name of Thomas Hoag; that she then thought him and still thinks him the handsomest man she ever saw. *Judgment.*

*IV. Defense.* Joseph Chadwick, who testified, that he had been acquainted with the prisoner, Joseph Parker, a number of years; that witness resides in this city, is a rigger by trade; that the prisoner worked in the employ of the witness a considerable time as a rigger; that prisoner began to work for witness in September, 1799, and continued to work for him till the spring of 1801; that during that period he saw him constantly; that it appeared from witness' books that Parker received money from witness, for work which he had performed on the following days, viz.: on the 6th of October, and 6th and 13th December, 1800; on the 9th, 16, and 28th February, and 11th March, 1801; that Parker lived from May, 1800, till sometime in April, 1801, in a house in the city of New York belonging to Capt. Pelor; that during that period, and since, witness has been well acquainted with the prisoner. *Judgment.*

*V. Defense.* Isaac Ryckman testified, that he was an inhabitant of the city of New York: that he was well acquainted with Joseph Parker, the prisoner at the bar, and had known him a number of years; that witness and Parker were jointly engaged in the latter part of the year 1800, in loading a vessel for Capt. Tredwell, of New York; that they began to work on the 20th day of December, 1800, and were employed the greater part of the month of January, 1801, in the loading of the vessel; that during that time the witness and Parker worked together daily; the witness recollected well that they worked together on the 25th day of December, 1800; he remembered it because he never worked on Christmas day, before or since; he knew it was in the year 1800, because he knew that Parker lived, that year, in a house belonging to Capt. Pelor, and he remembered their borrowing a screw for the purpose of packing cotton into the hold of the vessel they were at work at, from a Mrs. Mitchell, who lived next door to Parker; that witness was one of the city watch, and that Parker was also at that time upon the watch; and that witness had served with him from that time to the present day, upon the watch, and never recollected missing him any time during that period from the city.

Aspinwall Cornwall testified, that he lived in Rutger street, and had lived there a number of years; that he kept a grocery store; that he knew Parker, the prisoner at the bar, in 1800 and 1801; that Parker then lived in Capt. Pelor's house; that he lived only one year in Pelor's house; that Parker, while he lived there, traded with witness; that witness recollected once missing Parker for a week, and, inquiring, found he had been at work on Staten Island, on board one of the United States frigates; that, excepting that time, he never knew him to be absent from his family, but saw him constantly. *Judgment.*

*VI. Defense.* Elizabeth Mitchell testified, that she knew Parker, the prisoner at the bar, well; that in the years 1800 and 1801 Parked lived in a house adjoining to

one in which witness lived; that the house Parker lived in belonged to Capt. Pelor; that witness was in habits of intimacy with Parker's family, and visited them constantly; that Parker being one of the city watch, she used to hear him rap with his stick at the door, to awaken his family, upon his return from the watch in the morning; that she also remembered, perfectly well, Parker's borrowing a screw from her on Christmas day, in 1800; she offered him some spirits to drink, but he preferred wine, which she got for him; the circumstance of her lending the screw to him she was the more positive of, from recollecting, also, that it was broken by Parker in using it; that Parker never lived more that one year in Capt. Pelor's house, and from that time to the present day, witness had been on the same terms of intimacy with Parker's family; she therefore considered it as almost impossible that Parker could have been absent from town, any time, without her knowing it; and she never knew him to be absent more than one week, while he lived at Pelor's house. *Judgment.*

*VII. Defense.* James Redding testified, that he had lived in the city a number of years; that he had known Parker, the prisoner at the bar, from his infancy; that Parker was born at Rye, in Westchester county; that Parker, in the year 1800, lived in Capt. Pelor's house; that witness saw him then constantly, and never knew him during that time to be absent from town, during any length of time; that witness particularly remembered that, sometime in the beginning of the month of January, 1801, while Parker lived in Capt. Pelor's house, witness assisted Parker in killing a hog.

Lewis Osborne testified, that he had been acquainted with Parker, the prisoner at the bar, for the last four years; that witness had been one of the city watch: that from June, 1800, to May, 1801, Parker served upon the watch with witness; that, at first, Parker served as a substitute; that witness remembered that Parker, a few days after Christmas, in 1800, was placed upon the roll of the regular watch, in the place of one Ransom, who was taken sick; witness was certain it was in the period above mentioned, because that was the only time witness ever served upon the watch; that during the above period, witness and Parker were stationed together, while on the watch, at the same post; witness was certain that Parker, the prisoner at the bar, was the person with whom he had served upon the watch, and was confident that during that time Parker was never absent from the watch, more than a week, at any one time. *Judgment.*

*VIII. Prosecution.* Moses Anderson testified, that he had lived at Haverstraw, Rockland county; that he had lived there since the year 1791; that he knew the prisoner at the bar well; that he came to the house of the witness in the beginning of September, 1800; that he then passed by the name of Thomas Hoag; that he worked for the witness eight or ten days; that from that time till the 25th of December, prisoner passed almost every Sunday at witness's house; that during prisoner's stay in Rockland county witness saw him constantly; and if prisoner was the person alluded to, he had a scar on his forehead, which he told witness was occasioned by the kick of a horse; he had also a small mark on his neck (those marks the prisoner had); he had also a scar under his foot, between the heal and ball of the foot, occasioned, as he told witness, by treading on a drawing knife; *that that scar was easy to be seen;* that his speech was remarkable, his voice being effeminate; that he spoke quick and lisped a little (these peculiarities were observable in prisoner's

speech); that prisoner supped at witness's house on the night of his marriage in December, 1800; that witness had not seen prisoner until this day, since prisoner left Rockland, which was between three and four years ago; that witness was perfectly satisfied in his own mind that prisoner was Thomas Hoag.

Lavina Anderson testified, that she knew prisoner at the bar; his name was Thomas Hoag; that in September, 1800, he came to witness's house in Rockland county, and worked for her husband eight or ten days, then worked for Judge Suffrein; every Saturday night until the prisoner was married, he and a person who passed for his brother, came to witness's house and stayed till Monday morning; that witness washed for him; there was no mark upon his linen; that prisoner, if he is Thomas Hoag, has a scar upon his forehead, and one also under his foot; was certain of the mark under his foot, because she recollected that the person who passed as his brother, having cut himself severely with a scythe, and complaining very much of the pain, Thomas Hoag told him he had been much worse wounded and then showed the scar under his foot. Witness also testified, that about a year ago, after a suit had been brought in the justices' court in New York, wherein the identity of the prisoner's person came in question, witness was in town, and having heard a great deal said on the subject, she was determined to see him and judge for herself; that accordingly she went to prisoner's house, but he was not at home; she then went to the place where she was informed he stood with his cart; that she there saw him lying on his cart with his head on his hand; that in that situation she instantly knew him; that she spoke to him and when he answered she immediately recognized his voice; that it was very singular, shrill, thick, hurried, and something of a lisp; that Hoag had also a habit of shrugging up his shoulders when he spoke, which she also observed in the prisoner; that prisoner said he had been told she was coming to see him, and it was surprising people could be so deceived, and asked witness if she thought he was the man, to which witness replied that she thought he was, but would be more certain if she looked at his forehead; that she accordingly lifted up his hat, and saw the scar upon his forehead, which she had often before seen; that prisoner then told her it was occasioned by the kick of a horse. Witness added that it was impossible she could be mistaken—prisoner was Thomas Hoag. *Judgment.*

*IX. Prosecution.* Margaret Secor testified, that about four years ago she lived at Rockland with her father, Moses Anderson; that prisoner at the bar, Thomas Hoag, came to this house in September, 1800; that he remained in Rockland five or six months; that he had a scar on his forehead, that Hoag used to come every Saturday night to her father's to pass Sunday with them; that she used to comb and tie his hair every Sunday, and thus saw the scar; that witness married about two years ago, and came immediately to live in the city of New York; that after she had been in town a fortnight, she was one day standing at her door, when she heard a cartman speaking to his horse; that she immediately recognized the voice to be that of Thomas Hoag, and upon looking at him, saw the prisoner at the bar, and instantly knew him; that as he passed her he smiled and said, "How d'ye do, cousin?" that the next day he came to her house and asked her how she knew he was the man; witness replied she could tell better if he would let her look at his head; that accordingly she looked and saw a scar upon his forehead, which she had often

remarked upon the head of Hoag. Witness admitted she had mentioned her suspicions to her husband, and that her husband had told prisoner of it, and had brought him to the house. Witness added that she was confident prisoner was the person who passed at Rockland as Thomas Hoag.

James Secor testified, that he had been married about two years and a half; that he brought his wife to town about a week after his marriage; that he knew Hoag in Rockland, and had repeatedly seen him there; when he saw prisoner at his house in town, thought him to be the same person; witness's wife had mentioned to him that Hoag had a remarkable scar on his forehead, and when prisoner was at witness's house, he saw on his head the scar that his wife had described.

Nicholas W. Conklin testified, that he lived in Rockland county; that he knew the prisoner at the bar; that his name was Thomas Hoag; that he could not be mistaken; that Hoag had worked a considerable time for him; that during that time he had eaten at witness's table; that Hoag being a stranger, and witness understanding that he was paying addresses to Catherine Secor, witness took a good deal of notice of him; thought him a clever fellow; saw a great deal of him; lived in a house belonging to witness. When witness saw prisoner at this place, he knew him instantly; his gait, his smile, which is a very peculiar one, his very look was that of Thomas Hoag. Witness endeavored, but in vain, to find some difference in appearance between the prisoner and Hoag; he was satisfied in his mind that he is the same person. Hoag, he thought, was about twenty-eight or thirty years of age; he thought Hoag had a small scar on his neck.

Michael Burke testified, that he lived in Catherine street; that he formerly lived in Haverstraw, he saw prisoner several times at Haverstraw, before and after his marriage in December, 1800; that he was as well satisfied as he could be of anything, that prisoner was the same person he knew in Haverstraw, that about two years ago he met the prisoner in the Bowery, at the time of the Harlem races; prisoner spoke to witness, and said, "Am I not a relation of yours?" Witness replied, "I don't know." Prisoner said, "I am; I married Katy Secor." Upon *Cross-examination* witness admitted that he and prisoner had had a quarrel respecting witness calling prisoner Thomas Hoag; that the above conversation was after the trial in the justices court, and witness when asked if he was at the trial, said he was not, though when interrogated particularly whether he was not in the court room admitted that he was.

Samuel Smith was called, merely as to the character of one of the witnesses on the part of the prosecution, a Mr. Knapp, and testified that he bore an unexceptionable character.

Abraham Wendell testified, that he knew one Thomas Hoag in the latter end of the year 1800; he was then in Haverstraw; that he had been very intimate with him, and knew him as well as he knew any man; that he had worked with him, had breakfasted, dined, and supped with him, and many a time had been at frolics with him, and that the prisoner at the bar was the same man; that he had no doubt whatever about it. That about a year ago, witness being in this city was told by some persons that Hoag had beat the Haverstraw folks in an action wherein his identity had come in question; that witness told them he could know him with certainty; that they said they would send him down to him that day; that witness was aboard his sloop, saw prisoner at a distance of a hundred yards, coming down the street, and instantly knew him; prisoner came up to him and said immediately, "Mr.

Wendell, I am told you will say you know me;" to which witness replied, "So I do; you are Thomas Hoag;" that witness was as confident prisoner is the person, as he was of his own existence.

Sarah Conklin testified, that she lives in Haverstraw; that in September, 1800, a person calling himself Thomas Hoag was at witness's house, was very intimate there, used to call her aunt; is sure prisoner is the same person; never can believe two persons could look so much alike; would know Hoag from among a hundred people by his voice; Prisoner must be Thomas Hoag; had not seen prisoner since he left Haverstraw till the present day.

Gabriel Conklin testified, that he lived in Haverstraw; that he knew Thomas Hoag; that was at witness's house in September, 1800, and often afterwards; prisoner is the same person, unless there can be two persons so much alike as not to be distinguished from each other; prisioner must be Thomas Hoag; Thomas Hoag had a scar on his forehead and a small scar just above his lip, and prisoner had also these marks. *Judgment.*

*X. Defense.* James Juquar testified that he had known Joseph Parker, the prisoner at the bar, for seven years past; that he had been intimate with him at that time; that they had both worked together as riggers until Parker became a cartman; knew Parker when he lived in Capt. Pelor's house; never knew him absent from the city during this time, for a day, except when he was working on board one of the United States frigates, about a week at Staten Island. In the year 1799, prisoner hurt himself on board the Adams frigate, and then went to his father's in Westchester county, and was absent near a month; he was very ill when he left town; witness went with him, and brought him back again, before he was quite recovered; recollects Parker and some other company passing Christmas eve at witness's house the year that Parker lived in Capt. Pelor's house, which was in 1800.

Susannah Wendell testified, that she had known prisoner for six years past; that he married witness's daughter; knew him when he lived in Capt. Pelor's house. Parker's wife was then ill, and witness had occasion frequently to visit her; saw prisoner there almost daily; Prisoner, excepting the time when he was sick and went to his father's in Westchester, has never been absent from the city more than one week since his marriage with witness's daughter. *Judgment.*

*XI. Defense.* Here it was agreed between the attorney-general and the counsel for prisoner, that the prisoner should exhibit his foot to the jury, in order that they might see whether there was that scar which had been spoken of in such positive terms by several of the witnesses on the part of the people. Upon exhibiting his foot, not the least mark or scar could be seen on either of them.[†] *Judgment*

*XII. Defense.* In further confirmation of prisoner's innocence, there was adduced on his behalf one more witness:

Magnus Beekman, who testified, that he was captain of the city watch of the second district; that he was well acquainted with the prisoner, Joseph Parker; that he, Parker, had been for many years a watchman, and had done duty constantly

[†] Installment XI is of course neither testimonial nor circumstantial evidence. It is rather immediate, or direct, or real evidence, or again as Wigmore calls it, "Autoptic proference." Since, however, its persuasive value was for the defense we have as a matter of convenience included it among the testimony for the defense.

on the watch; that witness recurring to his books, where he keeps a register of the watchmen and of their times of service, found that prisoner, Joseph Parker, was regularly upon duty as a watchman during the months of October, November, and December, 1800, and January and February, 1801, and particularly that he was upon duty the 26th of December, 1800. *Judgment.*

*XIII. Verdict.* The jury, without retiring from the bar, found a verdict of not guilty. *Judgment.*

*Results.* The numerical results showing the distribution of the *Ss*' judgments on the 9-point scale for each of the installments are shown in Tables I–IV. Graphs showing the mass tendencies of their judgments

TABLE I

THE DISTRIBUTION OF JUDGMENTS IN EXPERIMENT 1

| Scale values | Installments of evidence | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | VIII | IX | X | XI | XII | XIII |
| 9 | 1 | 4 | 11 | 1 | | | | 4 | 8 | 5 | | | |
| 8 | 4 | 12 | 10 | 6 | 2 | 1 | | 5 | 12 | 9 | | | |
| 7 | 6 | 15 | 16 | 7 | 5 | 2 | | 7 | 11 | 7 | 1 | | |
| 6 | 22 | 10 | 7 | 11 | 5 | 5 | 7 | 10 | 7 | 2 | 1 | 1 | 1 |
| 5 | 15 | 9 | 6 | 19 | 21 | 17 | 10 | 20 | 7 | 14 | 16 | 4 | 3 |
| 4 | 1 | | | 4 | 10 | 9 | 15 | 2 | 2 | 8 | 2 | 6 | 2 |
| 3 | 1 | | | 1 | 4 | 13 | 14 | 1 | 2 | 3 | 12 | 12 | 10 |
| 2 | | | | 1 | 2 | 2 | 3 | 1 | 1 | 2 | 16 | 20 | 18 |
| 1 | | | | | 1 | 1 | 1 | | | | 2 | 7 | 16 |
| M | 5.8 | 6.9 | 7.3 | 5.5 | 4.9 | 4.5 | 4.0 | 5.6 | 7.0 | 5.4 | 3.1 | 2.4 | 2.0 |
| $Q_1$ | 5.2 | 5.9 | 6.4 | 4.9 | 4.1 | 3.2 | 3.1 | 4.9 | 5.6 | 4.4 | 2.2 | 1.8 | 1.3 |
| $Q_3$ | 6.4 | 7.8 | 8.4 | 6.7 | 5.5 | 5.2 | 5.0 | 7.0 | 8.1 | 7.7 | 4.8 | 3.4 | 2.9 |

for each of the installments are shown in Figs. 1–4. Since the steps in our scale are probably not equal, we have taken the median value as representing the central tendency, and the quartile deviation on each side of the median as representing the variation. The heavy central line in every figure represents the course of the median value of the *Ss*' judgments; the width of the figure represents the quartile deviation above and below the median.

An analysis in detail of the first experiment by reference to Table I and Fig. 1 will make the other tables and figures easily understandable. The first vertical column contains the distribution of judgments for Installment I (reading of the indictment). One *S* was convinced of guilt, 4 *Ss* had strong belief in guilt, 6 a fair belief in guilt, 22 a slight belief in guilt, 15 were in doubt, one had a slight belief and one a fair belief in innocence. The median is 5.9 which means roughly that the group as a whole with an average quartile deviation of 0.60 were led, merely by the indictment, to report a slight feeling of guilt.

The second column gives the distribution of judgments following the testimony of the first witness for the prosecution (a common-pleas judge who had known Thomas Hoag personally, who had married Hoag to Catherine Secor, and who was certain of his identification of the defendent). The distribution has changed significantly. Only 9 *Ss*, 18%, maintain doubt; all the rest are more or less certain of guilt. The third in-

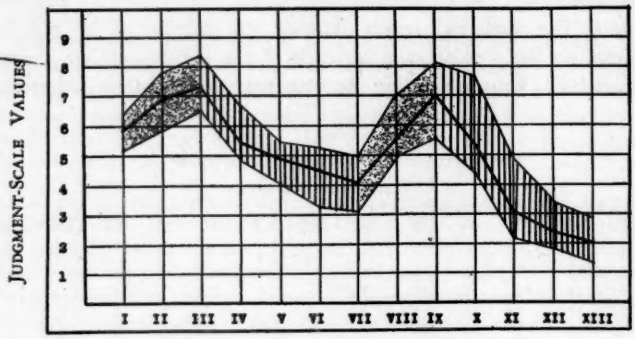


FIG. 1. RELATION BETWEEN JUDGMENTS AND INSTALLMENTS OF TESTIMONY IN EXPERIMENT 1

The central black line represents the median value of all the judgments for each installment. The width of the figure represents the quartile deviation above and below the median. The stippled portion shows the effect of the evidence for the prosecution; the barred portion that for the defense.

stallment also for the prosecution (the important witness being Catherine Secor, who married Hoag and was sure of her identification) changed the distribution in the direction of certainty of guilt although it had little effect on the median.

The next installment (IV) was the beginning of the testimony for the defense. It consisted in the testimony of Joseph Chadwick (who had known the defendent under the name of Joseph Parker and who testified that the accused had been in his employ in New York City during the time that the witness had known him). The distribution is in the direction of doubt. The next installment (V) carried the median to a point slightly below doubt and the next two installments, VI and VII, still further in the direction of innocence. Here, 14% of the *Ss* still have a slight belief in innocence; one is certain of innocence.

Installments VIII and IX contain further evidence for the prosecution. The former had more effect upon the change in distribution than any testi-

mony presented up to this time. Two witnesses testified to certain scars and to the existence of a mark on the neck of Thomas Hoag; one of these scars and the mark on the neck were visible on the prisoner. (The other scar, said to be visible on the bottom of Hoag's foot played an important part in the case later on.) One of the witnesses also testified to peculiarities in Hoag's voice which were also found in the prisoner. The general

TABLE II

THE DISTRIBUTION OF JUDGMENTS IN EXPERIMENT 2

| Scale values | Installments of evidence | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | I | II | III | IV | V | VI | VII | X | XI | XII | VIII | IX | XIII |
| 9 | 3 | 6 | 7 | 3 | | | | | | | | 1 | 4 |
| 8 | 21 | 19 | 17 | 6 | 2 | 1 | 1 | | | | 3 | 7 | 4 |
| 7 | 3 | 8 | 9 | 8 | 2 | 1 | | | | | 4 | 8 | 3 |
| 6 | 3 | 7 | 5 | 7 | 8 | | | 1 | | | 2 | 6 | 2 |
| 5 | 16 | 6 | 8 | 19 | 16 | 12 | 13 | 13½ | | 1 | 13 | 10 | 8 |
| 4 | | | | 2 | 13 | 21 | 10 | 8 | 9 | 7 | 8 | 1 | 8 |
| 3 | | | | 1 | 4 | 9 | 18 | 16 | 9 | 9 | 11 | 6 | 6 |
| 2 | | | | | 1 | 2 | 4 | 7 | 24 | 24 | 4 | 5 | 7 |
| 1 | | | | | | | | 1 | 4 | 5 | 1 | 2 | 2 |
| M | 7.5 | 7.6 | 7.6 | 5.6 | 4.8 | 4.1 | 3.6 | 3.4 | 2.3 | 2.2 | 4.4 | 5.4 | 4.4 |
| $Q_1$ | 5.2 | 6.3 | 6.2 | 4.9 | 4.0 | 3.5 | 2.9 | 2.7 | 1.8 | 1.9 | 3.1 | 3.2 | 2.8 |
| $Q_3$ | 8.1 | 8.2 | 8.2 | 7.2 | 5.6 | 4.7 | 4.7 | 4.7 | 3.2 | 3.1 | 5.3 | 7.1 | 5.5 |

effect of this testimony was to increase belief in the defendent's guilt although 40% of the *Ss* were in doubt. Installment IX contained testimony that corroborated statements made in Installment VIII, and also statements of witnesses that they recognized Hoag in New York City. Now 40% of the *Ss* were either convinced or had a strong belief in guilt, and the doubtful cases decreased from 40% to 14%.

Then followed more testimony for the defense. Installment X contained nothing new by way of evidence but had the effect of overcoming the evidence of installment IX and creating greater confusion in the minds of the jurors. The deviation from the median at this point was 1.7 which was the largest variation found in this experiment.

With Installment XI comes the most dramatic incident of the trial. Since a number of witnesses for the prosecution had testified to the presence of a scar on the foot of Thomas Hoag, it was agreed by the attorney-general and the counsel for the prisoner to exhibit the foot to the jury. When this was done no scar or any mark was visible. The effect was remarkable, as 60% of the *Ss* now have at least a fair belief in the prisoner's innocence; 32% are, however, still doubtful. The final evidence for the defense was that of a witness who from his records showed that Joseph Parker had performed his duty as watchman during the month of December 1800, and

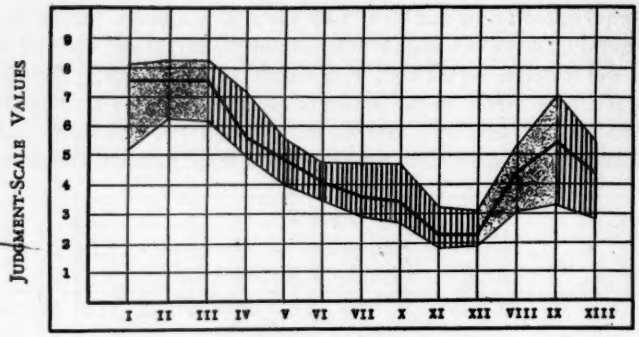


FIG. 2. RESULTS OF EXPERIMENT 2

particularly on the 26th of December. Now 78% of the *S*s have at least a fair belief in his innocence; only 8% are doubtful.

At this point the verdict of the jury in the actual trial was read and judgments were recorded to see whether the decision of the jury had any effect. The results showed that certainty of innocence was increased.

The fact that the median changes its direction as the evidence changes from prosecution to defense, or vice versa, led us to ask whether the final result might not have been influenced by the order in which the testimony was given. In the second experiment therefore we gave the first three installments as in the first experiment and then followed that with all of the

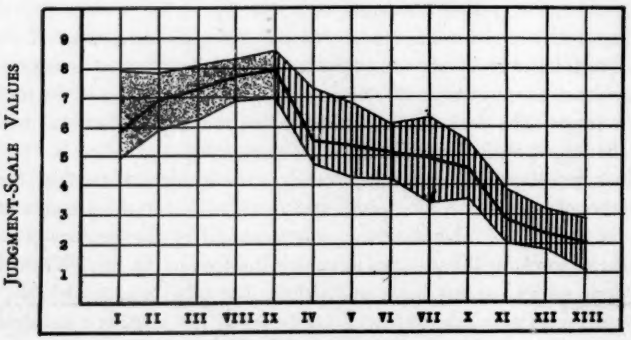


FIG. 3. RESULTS OF EXPERIMENT 3

testimony for the defense including installment XI. Then followed the remaining testimony for the prosecution. A glance at Fig. 2 will show that in so far as the first six installments are concerned the result is about the same as in Fig. 1. The continuation of the testimony for the defense, however, carried the judgments down to a median of 2.4 (a fairly strong belief

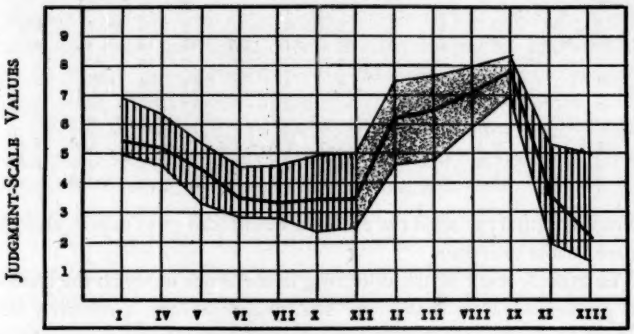


FIG. 4. RESULTS OF EXPERIMENT 4

in innocence). The remaining testimony for the prosecution then changed the median judgment to doubt.

In the third experiment (Table III, Fig. 3) all of the testimony for the prosecution was first given and then all of the testimony for the defense. The charts show that at the end of the testimony for the prosecution the median was 7.89, a strong belief in guilt; but as the testimony for the defense accumulated, the median judgment dropped to doubt but with wide variation and then at the end reached a point of 2.3—a strong belief in innocence.

In experiment 4 all of the testimony for the defense except the showing of the foot to the jury was given first, then all of the testimony for the prosecution, then the foot was shown. When the testimony was given in this order its greatest effect for the defense was a median of 3.33, but installments X and XII, still for the defense, actually raised the median by a small amount. The effect of the testimony for the prosecution was to raise the median to 7.77—a strong belief in quilt. Then with the showing of the foot the median dropped to 3.5—a strong belief in innocence. The slight rise in the upper quartile at installment VII in Fig. 3, and a similar rise in Fig. 4 at installment XII were the result in both cases of two judgments

TABLE III

THE DISTRIBUTION OF JUDGMENTS IN EXPERIMENT 3

| Scale values | Installments of evidence | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | I | II | III | VIII | IX | IV | V | VI | VII | X | XI | XII | XIII |
| 9 | 6 | 5 | 6 | 8 | 14 | 3 | 2 | 2 | | | | | 1 |
| 8 | 11 | 11 | 17 | 22 | 18 | 7 | 1 | 2 | 3 | 1 | | | 2 |
| 7 | 6 | 15 | 13 | 11 | 10 | 10 | 13 | 5 | 8 | 5 | | | 1 |
| 6 | 3 | 10 | 6 | 4 | 5 | 5 | 6 | 9 | 7 | 6 | 1 | 2 | 1 |
| 5 | 20 | 7 | 5 | 4 | 3 | 16 | 14 | 17 | 12 | 14 | 5 | 4 | 2 |
| 4 | 1 | 1 | 2 | 1 | | 5 | 10 | 7 | 7 | 12 | 10 | 3 | 1 |
| 3 | 1 | | | | | 4 | 4 | 7 | 7 | 4 | 13 | 11 | 7 |
| 2 | 2 | 1 | 1 | | | | | 1 | 6 | 6 | 17 | 25 | 21 |
| 1 | | | | | | | | | | 2 | 4 | 5 | 14 |
| M | 5.8 | 6.9 | 7.3 | 7.7 | 7.9 | 5.5 | 5.3 | 5.1 | 4.9 | 4.6 | 2.8 | 2.3 | 2.0 |
| $Q_1$ | 4.9 | 5.8 | 6.2 | 6.8 | 6.9 | 4.7 | 4.2 | 4.1 | 3.4 | 3.5 | 2.0 | 1.8 | 1.1 |
| $Q_3$ | 7.9 | 7.8 | 8.1 | 8.3 | 8.6 | 7.2 | 6.8 | 6.1 | 6.3 | 5.5 | 3.8 | 3.2 | 2.9 |

which are contrary to what the evidence would lead us to expect, and which we are unable to explain.

The general result of the difference in the order in which the testimony was presented was to change the final judgment from a relatively strong belief in innocence (Figs. 1 and 3) to a fair belief in innocence (Fig. 4), or to doubt (Fig. 2).

A study of all of the tables and figures will also show (1) that the mere indictment created a slight tendency to regard the defendent as guilty. (2) That the direction of the mass tendency as represented by the median is in general a function of the testimony, *i.e.* it tends to rise with the testimony for the prosecution and to fall with that of the defense. (3) That as evidence of a particular kind accumulates the central tendency continues to move in one direction but by relatively smaller steps. In other words there is a law of diminishing returns which, however, may be cut

TABLE IV

THE DISTRIBUTION OF JUDGMENTS IN EXPERIMENT 4

| Scale values | Installments of evidence | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | I | IV | V | VI | VII | X | XII | II | III | VIII | IX | XI | XIII |
| 9 | 2 | 1 | | | | | | | | 3 | 5 | | 2 |
| 8 | 3 | 1 | | | | | | 6 | 9 | 9 | 15 | | |
| 7 | 5 | 5 | 3 | | 1 | | | 9 | 7 | 10 | 7 | 3 | 1 |
| 6 | 3 | 5 | 3 | 1 | 2 | 5 | 6 | 3 | 1 | 3 | 1 | 3 | |
| 5 | 19 | 13 | 10 | 8 | 6 | 5 | 4 | 7 | 9 | 5 | 2 | 8 | 2 |
| 4 | | 6 | 7 | 8 | 5 | 5 | 4 | 1 | 3 | | 2 | 2 | |
| 3 | | | 6 | 10 | 12 | 7 | 9 | 4 | 3 | 2 | | 3 | 4 |
| 2 | | 1 | 3 | 5 | 6 | 10 | 9 | 2 | | | | 9 | 9 |
| 1 | | | | | | | | | | | | 4 | 10 |
| M | 5.3 | 5.2 | 4.5 | 3.6 | 3.3 | 3.4 | 3.4 | 6.2 | 6.5 | 7.1 | 7.8 | 3.5 | 2.2 |
| $Q_1$ | 4.9 | 4.6 | 3.3 | 2.8 | 2.7 | 2.3 | 2.4 | 4.6 | 4.7 | 5.8 | 6.9 | 1.9 | 1.3 |
| $Q_3$ | 6.9 | 6.3 | 5.3 | 4.6 | 4.7 | 4.9 | 5.0 | 7.3 | 7.6 | 7.9 | 8.3 | 5.3 | 5.0 |

across by a particularly strong bit of testimony. (4) That a particular bit of evidence occurring with other evidence of the same kind is more effective in some orders than in others or, in other words, the effectiveness of the same testimony depends in part upon its ordinal place. For example, the effect of Installment III in all four experiments is almost exactly the same; namely, a slight rise above II which it always follows; but the effect of Installment II in the first three experiments where it followed the indictment was never more than a single step, whereas in Experiment 4 where it followed six installments for the defense it changed the judgment by three steps. Furthermore, Installment IV was very effective in the first three experiments and had practically no effect in the fourth experiment; Installment XI was least effective near the end of all the testimony for the defense as in Experiments 2 and 3 and most effective after the testimony for the prosecution as in Experiments 1 and 4. In the case of this installment, however, the effect of the testimony was partially enhanced by a change in the testimony from prosecution to defense and was decreased by the limits of the scale, *i.e.* in Experiment 2 the median of the preceding judgment was 3.43; the judgment of XI could not therefore have differed more than 2.43 steps.

Taking the four experiments together we have a total of 178 *Ss* who have participated in the experiments, and 2136 judgments. These judgments were not evenly distributed over the nine-point scale. If they had been we should have had 237 judgments or 11% for each point. The actual distribution was as follows; judgment 5 (doubt), 24%; 9 (conviction of guilt), 5%; 1 (conviction of innocence), 2%; all the others ranged from 10%-13%, *i.e.* they approximated closely to the average. The larger proportion of doubtful judgments is, in view of the conflicting nature of the evidence, not surprising. The small proportion at the extremes of the scale reflects the indecisiveness of the testimony and an attitude of caution on the part of many individuals. As regards the former there was no one bit of evidence which all individuals regarded as incontestable, and consequently many individuals never reached the point of certainty, they could never say 'there is no other solution.'

Let us turn now to Table V which shows the judgments of 12 different individuals. These are taken from Experiment 4 as representative of the differences among individuals which are found in all of our experiments. The judgments of each of these *Ss* are represented in the table by the first 12 letters of the alphabet.

*A, J,* and *L* maintain doubt until the beginning of the evidence for the prosecution. *A* and *J,* however, as they afterwards admitted, did not in the beginning follow instructions. No matter what the evidence, they felt that

they should maintain the attitude of doubt until all of the evidence was in. Neither was able however to maintain this attitude until the end. *J*, at the beginning of the evidence for the prosecution decided that the case was one of mistaken identity and all subsequent evidence failed to do more than to change his certainty of innocence. *L* was honestly doubtful but with a slight belief in guilt. This feeling was heightened by the evidence for the prosecution until *L* became certain of guilt. For *A* and *J* the absence of the

TABLE V

THE JUDGMENTS OF 12 *Ss* IN EXPERIMENT 4.

| Installments of evidence | Subjects | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | A | B | C | D | E | F | G | H | I | J | K | L |
| I | 5 | 9 | 8 | 5 | 5 | 5 | 6 | 7 | 7 | 5 | 5 | 5 |
| IV | 5 | 5 | 8 | 4 | 6 | 5 | 5 | 5 | 7 | 5 | 5 | 6 |
| V | 5 | 4 | 7 | 3 | 4 | 3 | 5 | 7 | 5 | 5 | 4 | 6 |
| VI | 5 | 3 | 7 | 2 | 2 | 3 | 4 | 4 | 3 | 5 | 3 | 5 |
| VII | 5 | 3 | 7 | 2 | 2 | 3 | 4 | 3 | 3 | 5 | 3 | 5 |
| X | 5 | 3 | 6 | 4 | 2 | 2 | 3 | 6 | 2 | 5 | 3 | 6 |
| XII | 5 | 2 | 6 | 4 | 2 | 2 | 3 | 6 | 2 | 4 | 3 | 6 |
| II | 5 | 5 | 8 | 7 | 2 | 3 | 6 | 7 | 5 | 4 | 5 | 7 |
| III | 5 | 7 | 8 | 8 | 3 | 5 | 7 | 8 | 7 | 3 | 5 | 8 |
| VIII | 5 | 8 | 8 | 8 | 5 | 7 | 8 | 8 | 7 | 3 | 7 | 9 |
| IX | 7 | 9 | 8 | 9 | 8 | 8 | 8 | 8 | 8 | 4 | 4 | 9 |
| XI | 2 | 1 | 6 | 5 | 2 | 5 | 5 | 2 | 4 | 1 | 4 | 7 |
| XIII | 1 | 1 | — | 5 | 2 | 2 | 1 | 1 | 3 | 1 | 2 | 7 |

scar on the bottom of the foot led them to the judgment of innocence whereas for *L* it only decreased the certainty of guilt.

*B, D, E, F, G, I,* and *K* are all more or less gradually moved toward belief in innocence by the cumulative testimony for the defense. All move toward guilt as the testimony for the prosecution begins, and all except *K* had either a strong or definite belief in the guilt of the prisoner before the foot was shown. *K* was partially convinced of innocence during the last of the evidence for the prosecution because it seemed contradictory.

*C,* however, was so convinced of guilt by the indictment alone that in subsequent judgments he never got below 6. All testimony for the defense merely decreased his certainty of guilt. *H* was in many respects similar to *C*. He, however, considered the evidence for the defense as conflicting and this led him to believe that the prisoner might be guilty. This belief was strengthened by the testimony for the prosecution.

A comparison of the horizontal line at judgment IX and the same judgments at installment XI, when the foot was shown to the jury, is instructive. At IX all but two of the 12 *Ss* were fairly or strongly convinced of guilt. With the showing of the foot, four changed their judgments to innocence,

three changed to doubt, two changed to less certainty of guilt, one changed to only slight belief in innocence. It is clear that *B*, *E*, and *H* are easily influenced by their previous judgments, and *D*, *F*, *G*, and *I* are conservative in their judgments moving a step at a time or not at all with successive installments and ending in doubt.

We may summarize the individual differences by saying that the individuals themselves reveal different trends or determinations in their evaluation of the facts. They are four in number and any single individual may have two or more of them at any one time but in different degree. There is first in a few individuals a pre-determination, an attitude of doubt, which is assumed at the beginning and maintained as long as possible; this attitude may be characterized as a rule that one should not reach a judgment until he hears all of the evidence. Secondly, there is a tendency revealed by some *S*s to be easily swayed by new evidence. Thirdly, there is a tendency to be cautious, deliberative, to balance a present bit of evidence with other evidence already presented and, in some cases, even to anticipate the possibility of future evidence. Fourthly, there is a determination set up by a preceding judgment; this determination, common in some degree to all individuals, is revealed in two ways. (a) A tendency for all later judgments to be influenced by an earlier one. This is weakest in those who are easily swayed by new evidence, and it is strongest in those who reach so high a degree of conviction in one direction that subsequent evidence which might be expected to move his judgment in the opposite direction has little weight. (b) A tendency for an immediately preceding judgment to influence the next one. That is to say, no single installment, except the first, is judged in an absolute sense, as if it stood alone. Insead, the judgment expressed as a degree on the scale is made relative to the preceding judgment.

There was hardly a testimonial fact that was not judged as most significant by one or more persons. For most individuals, however, the showing of the foot to the jury and the absence of any scar was considered the most significant disclosure in the trial. Some individuals questioned the absence of the scar on the ground that it had been at least four years since the scar had been seen by any of the witnesses who reported it, and consequently it might have disappeared. Several *S*s were particularly impressed by the testimony that Hoag had scars on his forehead and that the prisoner had these scars. Still others regarded the testimony of the judge (Coe) as most significant not only on the basis of his character as a judge but also because he was certain in his identification. Some thought that Catherine Secor,

who had been married to Hoag, could not have been mistaken in her identification; others, however, mistrusted her testimony principally because of her statement that she had, at the time of her marriage, "thought him and still thinks him the handsomest man she ever saw." This was considered as flippant. Some thought that the Haverstraw witnesses as a whole were more reliable, and still others believed the one side as creditable as the other. One observer thought the case of the prosecution was clinched by the testimony that Hoag was identified in New York City after he had left Haverstraw, and another thought the case of the defense was clinched by the testimony that Parker was known continuously throughout the period that Hoag was said to have been in Haverstraw. Several were particularly impressed by the witness who brought his books into the court-room and from them testified that Parker had been on the watch in New York City when Hoag was married in Haverstraw.

We are now in a position to state what happens in the formation of an individual opinion. Under the conditions of our experiment in which earlier formed or accepted opinions have little influence as compared with political or social opinions, the individual is faced with one or more evidentiary facts. These facts are then evaluated. This evaluation is in part governed by the attitudes or determinations which we have just discussed; and in part by the weight given to a particular bit of testimony. This weight is also influenced by such factors as the character of the witness, the relative probability of the occurrence of the evidentiary fact, and its reasonableness or unreasonableness. Fnally, each fact is then evaluated in terms of the prisoner's plea. This is much more immediate than our analysis would lead us to suspect. Under the general determination to form an opinion the testimony is immediately apprehended as strong or weak and as significant to a greater or less degree. There is no evidence of a reasoning process, a drawing of inferences; the judgment is immediate.

*Discussion.* The fact that each judgment is conditioned not only upon the evidence presented in a single installment, but also upon previous judgments suggests a possible relationship to a similar phenomenon in psychophysical experiments. When, for example, an *S* is instructed to lift weights in succession and to say whether each one is 'heavy,' 'light,' or 'intermediate,' or to listen to a noise presented in a series of noises and to say whether it is 'loud,' 'soft,' or 'medium' in intensity, it has been found that the resultant judgment is not absolute, not based solely on the weight or the intensity of the noise, but also on some other judgment or, it may be, some other stimulus, or trace of a former stimulus.[8] There is, however,

[8] For a summary statement of these experiments and the references to them see,

an essential difference between these results and our own. In the psychophysical experiments *S* has no problem over and above the judgment of every stimulus as it comes along, he has no regard to the series as a whole. In our experiment, on the other hand, the successive installments are apprehended and judged with reference to the solution of a problem; the judgments are cumulative. For example, the judgment 8 at the end of Installment IX means: 'in view of the evidence thus far there is a strong belief in the guilt of the defendent.' The new evidentiary fact is not judged in and for itself as it is in the psychophysical experiments.

The question as to whether the cumulative nature of our series of judgments is in accordance with Wigmore's analysis is not so easy to answer. Wigmore naturally approaches the problem from the legal point of view. "Evidence," he says, "is always a relative term. It signifies a relation between two facts, the factum probandum, or proposition to be proved, and the factum probans, or material evidencing the proposition." The proof of the proposition is reached by way of two mental processes which he calls inference and proof. "Inference is the persuasive operation of *each separate* evidentiary fact, as to an *Interim Probandum*. Proof is the persuasive operation of the *total mass* of evidentiary facts, as to a Probandum." Since an inference is a mental process he suggests the following analogy to show how it works:

> "An Inference may be likened to a push given to a wheel-chair in which the thinker is sitting. The door of the room is open and the chair is pointed towards the door. Party A gives the chair a push in the direction of the door; Party B gives the chair another push in the same direction; and Party C then does the same. No one push suffices to get the chair to the door; possibly not all three. In the meantime Party M gives the chair a push away from the door, and Party N gives another similar push. Then comes Party Q and gives the chair a strong push towards the door. The movement of the chair at each single push is like an Inference. The final position of the chair is like Proof—the chair may or may not have reached the door. In studying the various kinds of evidence, therefore, we are not expecting Proof from any one piece, but only an Inference. No question of Proof arises until all the Evidence-pushes have been given."[9]

If we compare the analogy with our results, the points on our scale would mark the position of the chair after each push; and since Wigmore regards it as possible that several pushes in the same direction might send the chair to the door (*i.e.* proof may thus be reached), there is, so far, no dispute between us and our figures furnish an experimental illustration of the analogy.[10]

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C. C. Pratt, The time-error in psychophysical judgments, this JOURNAL, 45, 1933, 292-297.

[9] J. H. Wigmore, *The Science of Judicial Proof*, 1937, 7, 9, 10.

[10] Wigmore, it is true, has only one door (proof of guilt), whereas we have two doors (guilt or innocence), and in addition a barrier (doubt) between the two doors. The difference, however, is not important. In law the only question is whether the

Wigmore, however, does not seriously consider 'proof' in this sense. Proof, he thinks, as it is reached in the courts, requires something more than a sufficient number of pushes. It requires the sifting, the rearrangement and classification of the mixed mass of evidence so that the 'thinker' may consider not only each piece of evidence in relation to every other piece, but also the total mass as it bears upon the proposition to be proved.[11]

Since our experiment stopped when all the evidence had been presented, we have no experimental evidence concerning Wigmore's second form of proof. Nevertheless, except where the evidence is exceedingly complicated, we surmise that although the second form of proof may correct errors that might occur under the first form, and although it offers a logical justification for one's conclusions, it really is only another way, and it may be a better way, of considering the evidence. The fact that for the individual, proof is sometimes reached before the total mass has been received, and the further fact that our subjects were in general satisfied with their conclusions, and were ready to defend them without having the evidence summed up and discussed, supports this conclusion. It may also be said that in his second form of proof Wigmore relies entirely upon inference in a logical rather than a psychological sense. Each bit of reasoning is cast in a logical form and the conclusions drawn are viewed in their logical setting. Our *S*s did not 'reason' in this way. Every evidentiary fact was, under the determination to solve the problem, at once comprehended as bearing in a particular way upon the solution of the problem. The comprehension, the 'inference,' as our results and those of other experiments in thinking have shown, is itself dependent upon a large number of factors; there is no one psychological function of inferring.[12] Syllogistic thinking is another form of thinking, but one that is rarely encountered in the solving of the problems of every day life.

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defendent is guilty as charged; if there is even a reasonable doubt, the charge is not proved—there is no proof—the defendent is not guilty.

[11] It is, of course, not fair to push an analogy further than it was intended to go. There is, however, one aspect of Wigmore's analogy which, although perhaps not intended, is we think implicit in his theory. He pictures the 'thinker' as sitting passively in his chair and as pushed in the one direction or the other by the force of the evidence. In fact, the thinker is not passive. It is he who by the weight which he assigns to each evidentiary fact, in large measure determines its strength; he may in the exceptional case, even determine the direction of the push. The same evidence, therefore, might push one thinker in one way and another thinker in another way. This criticism, we believe, may fairly be brought against the Chart Method of the analysis of mixed masses of evidence which is the central theme of *The Science of Judicial Proof*.

[12] See G. Humphrey, Thought, in E. G. Boring, H. S. Langfeld and H. P. Weld, *Psychology: A Factual Text-book*, 1935, 463-482; M. Bentley, *The New Field of Psychology*, 1934, 288-317.

# DYNAMIC, PHYSIOGNOMIC, AND OTHER NEGLECTED PROPERTIES OF PERCEIVED OBJECTS: A NEW APPROACH TO COMPREHENDING

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The relations of comprehending and other 'higher' activities to perceiving, remembering, and imagining stand in need of clarification. Comprehending itself is still a vaguely defined kind of activity, with no adequate characterization of products. Many names for the products have been invented, to be sure; but such a term as 'concept,' 'meaning,' *'Gedanke,'* 'thought-object' or *'Bewusstheit'* implies little more than a logical designation which neither characterizes nor describes.

It is the purpose of the present paper to examine in a preliminary way certain products of activity which suggest extensions of apprehending in the direction of the 'higher mental processes.' These products stand somewhere between ordinary apprehended objects, on the one hand, and real issues of comprehending, on the other. They are therefore important in paving the way for a more exact characterization of comprehending itself.

It is difficult to conceive of any adequate functional account of a performance which has not been preceded by a careful description of its issues. This is our primary reason for approaching these intermediate modes from the side of product or functional outcome.

Now there is a danger that we shall become so engrossed in these matters that we shall forget the primary aim of the psychologist, which is to discover how the organism operates. In contemporary studies of perceiving, for example, description of things seen, heard, felt, tasted or smelled, and the relation of these products to the extra-organic stimulus have been so interesting that psychologists have forgotten functional problems altogether and are interested only in 'phenomena.' Now that descriptive categories have been worked out with some degree of success, however, attention might well be given to a modal account of the activities whence 'phenomena' issue.

We shall not be open to this criticism, however, for the activities and products which we propose to consider have scarcely been touched by psychological investigation. We may feel justified, therefore, in limiting our consideration to products of activity so long as we do not forget the ultimate purposes for which our information is to be used.

* Accepted for publication October 25, 1937.

A marked tendency prevails in current researches to work toward 'thought-objects' from the side of the better-known apprehended products. Since our descriptive vocabulary is limited almost entirely to the world of perceived, remembered, and imagined objects, it is obviously better to move gradually from them toward the imperfectly known topics and 'concepts,' devising a descriptive vocabulary as we proceed. Two salient and erroneous assumptions have here been made; the first is that we must propose a *genetic* series of objects leading up to the products of comprehending, and the second that there is only one important direction in which to proceed, *i.e.* in the direction of generalization and abstraction.

Our treatment differs from others in both of these respects. In the first place, our derivation of conceptual and topical products from apprehended objects will be a purely descriptive derivation involving no assumptions concerning genetic priority. Descriptive relationships are not only easier to establish; they should also precede any genetic account. The *a priori* assumption of some principle of genetical development (such as that of 'generalization') inevitably warps the descriptions or limits them to a single phase of the problems in question. We must therefore obtain our descriptions first and work out the genetic series later.

In the second place, it has been commonly believed that thought-objects differ from others *only* in terms of their general and abstract character. The result has been a failure to account, either genetically or descriptively, for the wide variety of products actually turned out by the organism. Our task is, then, to seek other directions of derivation, preceding from apprehended to comprehended products by many routes.

Some of the routes which we shall suggest have been experimentally verified, others are purely tentative and await factual support. Our primary purpose is to set problems rather than to solve them, and we shall try to set them in such a way as to avoid some of the dead-ends which at present block research. One of these dead-ends is the notion of generalization. As we shall attempt to show, the difficulty is not with the notion of generalization, but with the vague way in which the term is used and the tendency to regard it as a panacea for all problems of comprehending.

In pointing out the diversity of products which lie just beyond the apprehended products we shall perform a double service. In addition to paving the way for better understanding of comprehending, we shall also bring out many activities and psychological issues which are important in their own right. Generalized objects and the like have too often been regarded only as stepping stones on the way to concepts and topics. Too little attention has been given to their importance in actual performance.

Their importance for the organism at the time of production is quite independent of any real or hypothetical genetic relation to concepts. They are separate products which play their own parts. When we find that the generalized object is only one kind of product which suggests an extension of apprehending, we see that a great deal of what is commonly called 'understanding' may find here its proper place.

*Characteristics of apprehended objects and events.* Since we are taking the world of apprehended products as our stable region of reference, it will be well to summarize briefly the more important properties of these products. The spatial, temporal, and qualitative aspects of apprehended objects are universally recognized. In addition, configurational psychologists have maintained that the world of apprehending is organized into a set of segregated, organized, and integrated objects and events. That is to say, instead of an indifferent mass of quality-outspread-in-time-and-space, they stand for a spatial-temporal field in which objects and events appear as separate units internally organized and integrated.

All of this organization finds extended treatment in many researches.[1] Later we shall need to define more specifically such limits of organization as may be assigned to apprehending alone and to suggest extensions which imply additional functional resources. Here, however, we shall devote ourselves to a consideration of certain properties of apprehended objects, events, scenes, occasions, and situations which imply major divisions within the class of apprehensive activities. These properties, which enable us to distinguish perceiving from remembering or imagining, are designated collectively by Bentley as the *dating* of apprehended objects.[2] Dating has also received extended experimental investigation by Erb.[3]

Dating is a stamp or aspect which distinguishes one object from another quite independently of spatial, temporal or qualitative properties. This stamp is describable in two ways (or, as we might say, involves two different properties of objects): (1) as involving a temporal coefficient referring the object to a place in the time-continuum of the individual, and (2) as involving an additional aspect which we may term the *factuality* of the object. These two aspects are, as we shall see presently, interlaced in various complicated ways. Often we can separate them only artificially, but for the moment we shall select well-marked cases for illustration.

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[1] Cf. (among many others) K. Koffka, *Principles of Gestalt Psychology*, 1935; W. Metzger, *Gesetze des Sehens*, 1936; and M. D. Vernon, *Visual Perception*, 1937.

[2] M. Bentley, *The New Field of Psychology*, 1934, 73-79, 378-381.

[3] M. B. Erb, The memorial forms of apprehension, this JOURNAL, 49, 1937, 343-375.

The ordinary remembered object is definitely in *my* past, and the anticipated event in *my* future. But at the same time we find that these objects are *factual*. This event *did* happen, another *is going to* happen. Something else apprehended as future may be less factual or entirely fictional; 'this may happen' or 'this would never really happen.' Other combinations of these aspects may also occur.

We must emphasize that these stamps, temporal and factual, are stamps which the objects or occasions actually bear. They are not the products of secondary 'reflection' or 'reasoning.' They are directly there as properties of the object, and they are universal characteristics of the world of apprehended objects.

When we consider the temporal and factual sides of dating as independent variables, we see at once that many combinations are possible. Most of these combinations are found in Erb's list.[4] Others, for which we have only the rough evidence of everyday observation, were not called out by her method. In the following list we have attempted to suggest the range of forms which dating may take by listing those forms which are already experimentally verified as well as others which have not yet been experimentally considered. We shall indicate these unverified forms by means of the asterisk.[5]

KINDS OF DATING

*Factual objects and events*

(1)* static present
(objects simply 'are' with very little temporal patterning)
(2) present with temporal patterning
(events which go on in time, with a before and an after within the present)
(3) objects and events temporally continuous with the present
(a) the just-past ('memory after-image')
(b) the about-to-be (primary anticipation)
(4) objects and events temporally discontinuous with the present
(a) objects of the past (remembered objects)
(b) objects in the factual future (more remote anticipation)

*Fictional objects and events*

(1) temporally undated (phantasy)
(2)* dated in the *personal* past, present, or future
(suppositious past, 'what might have been,' *etc.*)

Most of the above headings are self-explanatory, and most have been carefully described and illustrated by Erb.

Some of the additions to and changes from Erb's list require discussion. We have subdivided the factual present somewhat differently. Her list includes under

[4] *Op. cit.*, 374.

[5] Some of the kinds of dating included by Erb in her list have been omitted. These primarily indicate aspects of objects other than dating proper (*e.g.* generalized and abstractive perceiving) which will be considered later.

perceiving "(1) Primary perceptive course with temporal coefficients," and "(2) Extension of 'present:' varying degrees of specificity among present members."[6] Both imply a temporal course, and the division is primarily in terms of the extent of the present. We shall regard both of these as belonging to our second heading under factual dating, the 'present with temporal patterning.' In addition, we have added the heading 'static present.' We do this because the 'permanent' objects of the apprehended world do not exhibit anything which could properly be called temporal course. These objects are 'just there.' Very often we may apprehend such a permanent object in different ways at different times, but the object remains permanent, it is our performance which is apprehended as changing. The events of the second sub-heading exhibit objects which *themselves* change in time, and the presentness of the event includes within it a before and an after. The event *goes-on* whereas the permanent object *is*.

We have also made an addition to the class of fictional objects, not only because it is another kind of fiction which should be investigated, but also because it has an important bearing upon certain problems of 'thinking.' This heading should suggest that hypothesis and supposition are by no means limited to thinking. We can 'suppose' apprehensively as well, not only objects and events which are divorced from any temporal order, but also objects and events which bear a peculiar relation to our personal past, future or present. The objects are fictional, but at the same time they are plausible and not fantastic. We use the subjunctive in describing many of these events.

The question of the nature and importance of the *personal* aspect of the temporal frame in which these objects are placed must be postponed until we are ready to consider the coordinate problem of *impersonal* frameworks. Objects may also be dated in an impersonal temporal order, but this will be considered along with the other extensions of apprehending.

This discussion of dating, incomplete as it is, should at least indicate something of the varied nature of apprehended objects and events. It should also show that if we are to describe such products adequately we must go beyond spatial, temporal, and qualitative aspects of the objects themselves, and must consider these in relation to the temporal order of the individual, and in terms of their factual and fictional character which cannot be described in spatial or temporal terms at all.

*Inspecting and examining.* While our present interest is primarily in products, and although the kind of performance called *inspecting* or *examining* does not necessarily add any new kinds of product, we must make an excursion into this topic because of a frequent confusion with comprehending and thinking. Careful characterizations of this kind of activity are given by Bentley and others (notably Hunt).[7] In addition, Bulbrook has shown by experiment the rôle which inspecting plays in the

[6] *Op. cit.*, 374.

[7] M. Bentley, *op. cit.*, 266-274; J. McV. Hunt, A functional study of observation, this JOURNAL, 47, 1935, 1-39.

so-called 'insightful' solving of problems.[8] We therefore need give the reader only a reminder of the nature of this activity. It consists essentially in an apprehensive type of performance which is temporally ligated and knit together by a task. It is, in other words, apprehensional performance with a marked forward-thrusting or determined course. Out of it come details, relations, and reorganizations of apprehended objects and events.

*Gestalt* psychologists have worked out certain 'configurational laws' which determine perceptual organization and have emphasized that these laws hold only when the observer takes a naïve or passive attitude toward the object. Thus a figure which is described geometrically as a rectangle with two diagonals is also seen in that way in accordance with the 'law of continuity' and the 'law of good figure.' It 'requires more effort' to see the same drawing as a view of a pyramid or pointed roof from a point directly above the apex. In fact, it may be that there are other organizations which can only be 'thought out' or 'understood' and cannot be perceived all at once, even under a task.[9]

We meet with the same distinction between direct perception and the products of secondary 'reflection' in many topics of experimental study. In discussing the factors of space-perceiving, Vernon differentiates between the immediate perception of spatial relations and "secondary, ideational processes of judgment."[10] Almost universally, however, such 'understanding' or 'secondary process' is brought into the discussion of perceiving as a variable which must be eliminated. It never receives treatment as a topic of importance in its own right. It is regarded as a negative case, a case where certain relationships between stimulus and properties of the perceived object do not hold.

Since we are interested here in the relations between comprehending and apprehending, we must come to terms with such processes as 'judgment' or whatever the activity is to be called. Whether or not we can throw light upon their nature, we must at least consider them as a problem.

We may suggest that in most, if not in all, such performances the product turned out is not different in any essential respect from other products of ordinary apprehending. The difference probably lies in the mode of activity itself. The product is similar in kind to ordinary products of perceiving, but the temporal course of the activity has changed; it has more of the determined thrust of performance which we have referred to

[8] M. E. Bulbrook, An experimental inquiry into the existence and nature of 'insight,' *ibid.*, 44, 1932, 409-453.

[9] H. Kopfermann, Psychologische Untersuchungen über die Wirkung zweidimensionaler Darstellungen körperlicher Gebilde, *Psychol. Forsch.*, 13, 1930, 293-364.

[10] M. D. Vernon, *Visual Perception*, 1937, 68.

as inspecting. Under the influence of a task the organism reorganizes, changes emphasis, and turns out a different product from that which is ordinarily determined by the stimulating conditions at the moment. The fact that the organism freely uses words as an aid in such reorganization does not imply that the performance is no longer an apprehensive or inspectional activity. It simply means that words may serve many purposes. They do not necessarily imply comprehending or thinking.[11]

Kopfermann suggests that we can understand certain figural relationships which have no counterpart even in a reorganization of a figure under a task. We can 'understand' that a certain group of lines 'represents' a perspective drawing of a certain solid object even though we cannot 'see' it that way even under a task.[12] This statement requires further consideration.

In the first place, Kopfermann's statement is obviously true, but it does not describe the product nor tell us how the organism accomplishes this result. The suggestion which we wish to make is not to be regarded as a solution for all kinds of such 'understanding' of figural relationships. It is not meant to cover, for example, Kopfermann's 'understanding' of three dimensional projections of four dimensional bodies.[13] Instead we restrict ourselves to much simpler cases of 'understanding.'

Let us state the problem more concretely. Suppose we consider the geometrical figure previously mentioned, a rectangle with the corners *a*, *b*, *c*, and *d*, and two diagonals which cross at *e*. Suppose also that the observer is able to *see* a pyramid with the rectangular base *abcd* and the apex *e*, but that he cannot *see* the triangular pyramid which has *abc* as a triangular base and *d* as its apex. Yet he is able to 'understand' that the drawing could represent such a figure. How is this accomplished?

The difficulty is of course with the word 'see' as well as with the word 'understand.' The above statement may be interpreted as meaning that the triangular pyramid could not be perceived as a stable product, that it was never perceived at all, or that it was perceived only piecemeal and never all at once. The writer is convinced that in most cases it is either the first or third of these possibilities which holds.

We must therefore take account of the temporal course of the performance as well as of its final issue. By inspection, we may reach products by breaking the figure up into bits, seeing one set of lines at one time as a

[11] For a further discussion of the rôle of such verbal accompaniments see T. A. Ryan, Mathematical objects and symbolizing, this JOURNAL, 51, 1938, 290-291.
[12] Kopfermann, *op. cit.*, 332.
[13] *Ibid.*, 332-335.

triangular base, and then, later, another set of lines as forming the apical portion. Somehow out of such a progression of single products, the organism is able to turn out a single, composite product at the end. This product may be held together by comments, imagined figures, or simply by one of the partial products. It is certainly not necessary to postulate topical comprehending as a necessary adjunct to the performance.

To be sure, this long-term function with its searching or determined forward-thrust and composite product is closely related to comprehending. Perhaps we might even list it as a simple sort of comprehending on the basis of its functional characteristics. But the resemblance is almost entirely on the side of the activity itself; the products cannot be classed as topics or concepts, belonging as they do to the general variety of apprehended objects. Those who would call the performance 'understanding' oversimplify by restricting the number of activities which they place at the disposal of the organism, and by ignoring the important difference in the nature of the products of topical comprehending and this activity which we are considering.

This is a problem which awaits new techniques of study. It is important in many contexts. It is first of all an important general problem of function. What is the status of the 'final' product in such a step-wise procedure? What is the nature of such a product? and How is performance integrated in this way?

Secondly, it is a problem of great importance in certain special topics. It is obvious that the procedure we have described is of special significance in geometrical 'thinking.' It is probably of more importance than 'direct' perceiving under the so-called configurational laws. As a matter of fact, the configurational laws often constitute a hindrance rather than an aid in the solution of geometrical problems.

In what follows we shall, for the sake of brevity, speak largely in terms of more stable products which are 'all there at once.' As we do so, however, it must be remembered that many of these products may be found to come out of the step-wise activities just now discussed.

*Objects and events intermediate between apprehended and comprehended products.* We come now to the principal topic of this discussion, objects which imply some extension of the apprehensive activities in the direction of comprehending. Below we have listed five major categories of these 'intermediate objects' (as we shall term them for brevity's sake). Some of the categories are so large that we have also indicated certain divisions within the major groups. Each heading represents a group of objects which indicates one of the directions of derivation, or one route to be traced

out, between apprehended objects on the one hand and 'though-objects' on the other. On some of these routes we shall be able to suggest certain important way-stations, and perhaps even the relative distances of these stations on the way. For some of the others we can do no more than to give a few examples. Occasionally we may discover certain products of comprehending proper which seem intimately related to certain of the intermediate objects.

It will be seen at once that each of the groups under *(3)-(5)* is markedly different and distinct from the others. There may be other groups of objects falling between our categories. The important thing here, however, is to emphasize the divergence of the ways and the inadequacy of a derivation in a single direction.

INTERMEDIATE OBJECTS

*(1)* Extensions, integrations, and schematizations of spatial and temporal properties of objects
*(2)* Spatial and temporal frameworks
  (a) personal frames
  (b) impersonal frameworks
    (i) geographical and historical
    (ii) microscopic
*(3)* Objects with physiognomic and social properties
  (a) animate physiognomic objects
  (b) inanimate physiognomic objects
  (c) human groups and institutions (as apprehended or otherwise produced by individual activity)
*(4)* Generalized, classified, and ordered objects
  (a) generalized objects
  (b) objects as classified and ordered
*(5)* Causal and dependent relations of objects

*(1) Extensions, integrations, and schematizations of spatial and temporal properties.* This group is not entirely separate from the second. Because of certain differences in emphasis and in the problems involved, however, it is more convenient to treat it separately, for in it we are dealing with spatial and temporal *characteristics,* while in the second group we shall deal with *frameworks* or spatial and temporal *orders.*

Since extension, integration, and schematization all go on at once and are essentially only different aspects of single objects and events, we may use the term *consolidation* to cover all three.

The simplest of these consolidations is probably a combination of remembered, perceived, and anticipated events into a single event-unit. All three activities go on more or less at once and turn out a single unified product. This kind of consolidation is probably very important in the sophisticated 'hearing' of such a large musical work as a symphony. The organism does more than just listen to the melodies and chords as they

come. Each is integrated with what has gone before, and the listener also goes ahead of the performers to anticipate what is coming.

Qualitatively or analytically, of course, the total event-unit—the whole symphony—is never apprehended all at once. Actually we have something in such consolidations which borders upon a temporal framework of the kind to be discussed a little later. The present sequence of notes, phrase or bar of music is, however, something more than 'just these notes;' it is a-part-of the larger unit. The larger event-unit is a composite of progressive products built up gradually by all of the apprehensive activities working together, just as the products of inspecting are often a progressive product of a series of separate reorganizations of the pattern. Less extensive melodic and rhythmic units are probably produced in the same way.

At a further remove from simple apprehending, the total event-unit may be so condensed or foreshortened that all that is qualitatively present is a schematic or partial pattern which 'represents' the whole. The first bars of Beethoven's *Fifth Symphony* are more than a series of notes. They are, for one familiar with that work, a schematic representation of the whole first movement of the symphony. Similarly we have schematic or conventionalized drawings which serve the spatial integration of objects.[14]

*(2) Spatial and temporal frameworks.* Of course, all apprehended objects are set in some kind of spatial-temporal framework. They are always oriented in some way with respect to other objects and events or with respect to the self-object. The mere existence of a spatial or temporal framework is not, therefore, evidence of any extension of the apprehensive functions. These frameworks are, however, usually 'silent'—to use Köhler's term—and usually determine activity without being explicitly or clearly apprehended. We find that the organism, on occasion, extends, makes more explicit, and changes the points of reference of these frameworks in such a way that something more than mere apprehending is implied. These changes are our special concern here.

First we find it useful to distinguish between a *personal* and an *impersonal* frame of reference. In the former, the self-object is a special central region of reference both in time and space. In the latter, space and time are organized about some other central region or are completely 'homogeneous,' *i.e.* without a point of reference. This distinction is not to be confused with the distinction between 'subjective' or 'phenomenal'

[14] C.f. W. S. Neff, Perceiving and symbolizing, this JOURNAL, 49, 1937, 376-418. It is doubtful whether such 'schematic representations' are to be called symbolical in any useful sense of the word. (Cf. T. A. Ryan, Mathematical objects and symbolizing, this JOURNAL, 51, 1938, 283-304.)

and 'objective,' 'physical,' or 'real' space and time. Both of our frames are 'phenomenal,' *i.e.* they are products of psychological activity. The difference between them is a phenomenal difference, and not epistemological or logical.

Stern has also used the terms 'personal space and time,' but the force of the word *personal* is, in keeping with his 'personalistic' approach, different from our usage.[15] His notion seems to be that even on the level of physiological activity the organism (*Person*) lives in a world which is different from the objective world. The world of stimuli and of energy-exchange between the organism and its surroundings is also organized with a central region of reference, the organism. Since the phenomenal world is a 'mirroring' of this personal world it also is organized with the self as a point of reference. For this reason, Stern chooses most of his examples of 'personal space and time' from apprehended occasions, but at the same time he emphasizes the possibility of a similar polarity in the world which is not 'experienced,' *e.g.* the effect of past history of the organism which is effective without memory.[16]

Katz adopts Stern's notion of personal space in its entirety.[17] It is useful in dealing with lower animals to extend the notion of the personal polarity of space and time to the whole environment of the animal without making assumptions concerning the phenomenal nature of space and time for the animal.

We are primarily interested here, however, in the spatial framework as perceived or otherwise produced by psychological activity. We need not concern ourselves with Stern's wider usage of the term 'personal.' In using the term, then, we shall imply the adjective 'phenomenal.'

*(a) The personal frameworks.* As we have said above, all apprehended objects are set in some sort of framework, and probably the simplest of these is of the personal kind. Objects are oriented with respect to *us* and we remember objects in our *personal* past. Even this personal frame of reference may, however, be extended and integrated in ways which suggest some extension of the apprehensive activities.

First, in a way resembling the organization of events into a connected and integrated sequence, the personal temporal and spatial frames are organized into continuous and articulated units. My past is a single organized unit into which remembered events are set, and the spatial frame is similarly organized.

Secondly, the personal frameworks are extended outward at their boundaries in a *non-qualitative* or unpatterned fashion. Köhler's illustration of the fact that one is 'aware' of a precipice behind one, even though it is not actually apprehended fits in here.[18] In other words, the personal framework is not only continuous; it extends beyond what we apprehend. In a sense, we "see the other side of the mountain" without qualitative filling in the product, or at least with only sketchy and incomplete filling.

*(b) The impersonal frameworks.* Actually, of course, we have a constant intermingling of personal and impersonal frameworks. In many instances, nevertheless,

[15] W. Stern, *Allgemeine Psychologie auf personalistischer Grundlage*, 1935, 123 ff.
[16] W. Stern, *op. cit.*, 136.
[17] D. Katz, *Animals and Men*, 1937, 95, 131 ff.
[18] Described in K. Koffka, *Principles of Gestalt Psychology*, 1935, 322.

greater emphasis is laid upon the one or the other. It is instances of this latter sort which we now seek.

Turning back for the moment to the personal frames, we find that in them the temporal reference is well integrated, unified, and organized. For evidence we have only to point to the personal order implied in dating remembered and anticipated objects. The personal-spatial framework is, on the other hand, not so highly organized, and not so sharply set off from the impersonal order. The adult organism probably apprehends objects in a purely personal spatial frame only under special circumstances. The artist, however, tries to rule out the impersonal aspects of an object's orientation. As he draws, 'the look of the object from here' is an important aspect of his perception.[19]

'Independent' shape and orientation of an object constitute the simplest evidence of an impersonal frame of spatial reference. These are widely present in perceiving. A much more highly organized and integrated kind of impersonal frame is to be found in the compass or geographical orientation of objects. This is usually taken so much for granted that we scarcely notice it. When it is erroneous or especially striking it comes out more clearly. Cases of illusory orientation show us that objects are often set in a geographical order. Implicitly the street down which I walk is a 'north and south' street, even though the sun may tell me that I am going east.[20] In other words, the street is not only oriented with respect to me as an object or with respect to other streets, but it is also placed with respect to some kind of independent geographical order. We only notice this qualification clearly when it leads us astray.

Temporal settings of this impersonal kind are more difficult to discover. The suppositious past, present, and future, which we have mentioned as forms of dating, are still very close to the personal kind of temporal orientation. Such objects and events differ from remembered and anticipated events primarily in their *factual* character rather than in their temporal setting.

The best examples of an impersonal temporal setting are probably to be found in the imagining which accompanies reading (Bentley's *imaginal accompaniment*).[21] Such imagined events may, of course, be dated in many ways, but often they are dated in an *historical* fashion quite independent of the personal past or future. Napoleon at Elba is dated definitely in the past as I read of him, yet he has nothing to do with my own personal past. I do not remember the past times when I have read about him. Napoleon himself *as imagined* is of the past.

We also find examples of an impersonal *spatial* setting in such imaginal accompaniments. Italy may be imagined in its geographical relationships to other countries, again a setting quite independent of the self-object. There is often, however, a peculiar kind of orientation with respect to the self-object which is mixed into this geographical setting. Quite often the imagined place is in a certain direction from *here*, and there is a tendency to point. New York is 'over that way,' Canada is

[19] R. H. Thouless, Phenomenal regression to the real object (I), *Brit. J. Psychol.*, 21, 1931, 339-359. In this study of the constancy of shape, the *Os* were performing under this 'artistic' instruction. This experiment demonstrates that even this instruction does not result in drawings exactly equivalent to the retinal pattern.

[20] A. Kirschmann, Über eine Orientierungstäuschung, *Zsch. f. Psychol.*, 100, 1926, 244-253; F. Baumgarten, Die Orientierungstäuschungen, *ibid.*, 103, 1927, 111-122; David Katz, *Animals and Men*, 1937, 131 ff.

[21] Bentley, *op. cit.*, 85-87.

'up there,' and so on. This is a kind of setting which may lie somewhere between the personal and impersonal kinds. It is as though the self-object were placed in a geographical continuum, rather than that the places are oriented with respect to the self-object.

All of the above kinds of spatial and temporal frameworks are more or less implicit and are noted only under abnormal circumstances. We also find, however, that upon occasion the setting or framework itself becomes a central product with the objects in the frame more or less incidental or even lacking. The framework is the object of consideration, and the object may be mere 'filling.'

Here we have moved to a more complicated kind of performance. Just what this activity is like is a question which awaits more exact description of the frameworks and their mode of production. We also come upon such complicated problems as the 'representation' of spatial and temporal orders by means of abstract depictions or schemata, problems which await further investigation. Certainly the use of such independent frameworks and schemata is very important in a great many activities of the organism, and they will repay a great deal of study.

*(3) Physiognomic and social properties.* We have deliberately avoided the use of such terms as 'significance' or 'meaning' in designating our next group of intermediate objects. It has in the past been assumed that such properties of other persons as 'intelligent,' 'angry' and 'pleased,' are meanings which the organism derives from the perceived pattern of face and posture, and are something separate from the perceived pattern itself. The assumption is based upon *a priori* notions as to what perceiving can accomplish. Since 'being pleased' is not 'really' a property of a person's face, we assume that we cannot possibly perceive such a property.

We cannot grant such an assumption until it has been descriptively verified. Our discussion of the fictional or factual character of apprehended objects has made it evident that even the most common apprehended objects cannot be described entirely in terms of shape, form, orientation, temporal progression, and the like. Yet the fictional character of phantasy is inherent in the imagined events themselves. It cannot be considered as a secondary 'meaning,' or 'significance.' Similarly, there is no valid reason why 'being pleased' is not just as inherent in the apprehended person as color of hair or length of nose.

The *Gestalt* psychologists have laid stress upon this same point of view. Koffka, in fact, believes that many of these physiognomic properties are inherent in the very early perceivings of the infant.[22] He presents many

[22] K. Koffka, *The Growth of the Mind* (2nd ed.), 1931, 149.

arguments against the notion that the affective properties are 'read into objects' by projecting one's own feelings into them.[23]

Although there may be many differences of opinion as to how widespread physiognomic properties are in common apprehending, it is certain that some, at least, are very common. Certain others indicate, however, a more elaborate form of activity, and may therefore be classed as intermediate. Arnheim's study, for example, represents an attack upon very complex physiognomic characters—the indication of character by handwriting, matching poetry to portraits of authors, and the like.[24]

We have not tried to distinguish between physiognomic objects in terms of complexity or supposed functional origin. We do not know enough about them as yet. We can, however, pick out certain general kinds. The second division above (p. 637) emphasizes the fact that physiognomic properties are by no means limited to persons. Inanimate objects may often have characteristics of the same kind. Thus the weather is 'threatening' and a building is 'gloomy,' 'cheerful,' or 'forbidding.' Such dynamic properties as the use of an object may also be classed here. The dollar bill is an object with which I can pay, a book may be a 'weight to hold my papers down,' and a hammer 'a thing for pounding.' As these examples indicate, some of these properties are only temporary, others (as the use of money) are relatively stable and permanent.

While we cannot discuss here all of the problems of 'feeling' and 'affective tone,' we may introduce the suggestion that many of these problems may be reinterpreted in a useful way in terms of physiognomic characters. It is the object itself which is 'pleasant,' or 'unpleasant,' and these terms may be grouped with all of the other physiognomic terms. The only context in which we can speak of 'subjective feelings' with any accuracy is that of the apprehension of bodily state. A great deal of the work on 'feeling,' especially in the apprehension of fairly complicated objects must be discussed in connection with our present topic.[25]

The experiments of Usnadze and of Fox, in which *Os* were asked to match nonsense words and nonsense figures, may also furnish an illustration of such properties of inanimate objects.[26]

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[23] K. Koffka, *Principles of Gestalt Psychology*, 1935, 326.

[24] R. Arnheim, Experimentell-psychologische Untersuchungen zum Ausdrucksproblem, *Psychol. Forsch.*, 11, 1928, 2-132.

[25] In the studies of K. Hevner in music, we find a wavering between the terms 'affective character' and 'expression' in music. The latter term seems to express more adequately the nature of these secondary characters of music. Her work can very readily be fitted into the total picture of physiognomic properties. K. Hevner, The affective character of the major and minor modes in music, this JOURNAL, 47, 1935, 103-118; Experimental studies of the elements of expression in music, *ibid.*, 48, 1936, 246-268.

[26] D. Usnadze, Ein experimentelle Beitrag zum Problem der psychologischen

Those instances in which they found that a nonsense word and a figure were matched because of some sort of 'general impression,' because they were both 'unpleasant,' or where the observer simply could not give an adequate reason for his choice are the ones in which we are particularly interested. It is possible that these matches are made on the basis of some physiognomic property of the objects which cannot be adequately described by the observers. Both the nonsense word and the figure may have some dynamic property, for instance, in common. This, of course, is only a conjecture. The statistical demonstration of agreement between observers in making the matches neither supports nor contradicts such a contention. The development of a terminology for description is the only way the problem can be solved.

The writer has suggested in a previous paper another field into which the notion of physiognomic properties of inanimate objects may be extended.[27] He implied there that numerical and algebraical signs and characters may not be, in actual practice, symbolical in the sense that they stand-for or 'mean' something else. Our reports in that experiment indicated that the signs and characters *themselves* may have many properties beyond the mere spatial, temporal and qualitative pattern. That is to say, 'being a squared number,' 'divisible by 3,' and the like, are often properties which inhere directly in the spatially and temporally qualified object.

The notion that such signs and characters are *per se* 'symbolical' is, however, so deeply rooted that one can scarcely hope to overcome the prejudice by a single argument. After experimental studies of the more obvious kinds of physiognomic property have multiplied, it may be more feasible to extend the notion of physiognomic property to these more complex objects.

Before turning to our third category (groups-as-socialized) we may remark that the physiognomic characteristics of other persons are most frequently apprehended in a social situation. Were we interested in their genesis, we might even hazard a guess that their origin is to be found in the socialized activities of the organism. At the adult level, however, we find that such properties may or may not be socialized; *i.e.* they may or may not be apprehended as referring to the self-object. Certain permanent attributes of other persons, such as intelligence, 'flightiness,' and the like, are rarely socialized. The person is not apprehended as 'intelligent with respect to this particular social setting' but simply as an 'intelligent person.' We may, if we like, subdivide our group of personal properties with respect to socialization.

Approaching the same problem from another side, we find that socialization of groups may be apprehended (or whatever the activity may be) in two different ways, corresponding to our personal and impersonal temporal frameworks. That is to say that the group may be socialized with respect to *me* as a self-object, or the group may be socialized independently of me. In the second case, I am not a member of the group.

Grundlagen der Namengebung, *Psychol. Forsch.*, 5, 1924, 24-43; C. W. Fox, An experimental study of naming, this JOURNAL, 47, 1935, 545-579.

[27] T. A. Ryan, *op. cit.*, 293-295.

The two situations may be combined in various ways. At one extreme there is the emotive predicament where the self is definitely the most important point of emphasis in the situation. At the other extreme, we find the self-object as a 'detached observer' and bystander, not a part of the social situation at all.

We emphasize this second, impersonal observation of socialization because it may furnish an important link in the derivation of certain important topical products of comprehending. A great deal of our political, economic, and sociological comprehending and thinking deals with institutions, groups, and social products which have become topics. Some of these products are 'anchored' in some building or place, others are almost completely independent of spatial-temporal objects. Some are tangled in complicated ways with insignia and other so-called 'symbols.' We may hope that further understanding of the apprehended group-as-socialized may aid our approach to these complicated products of comprehending.

*(4) Generalized, classified, and ordered objects.* The problem of generalization has, of course, been dealt with much more extensively in the experimental literature than have the problems discussed in the preceding sections. Much of the research available, however, is not relevant to our discussion of generalized *objects,* dealing as it does with generalization of *response.* By the term *generalized object* we do not refer to behavior 'in response to the object,' but to properties of the object itself as apprehended. We shall use the term only where the object possesses some attribute of 'generality,' where it is an object of this *kind* rather than *this particular* object.

In behavioristic experiments 'concept formation' is defined as the development of a generalized or 'symbolical' response to a common feature of different objects.[28] Applying a common name to two different objects is an example of such a 'concept.' Since, in these experiments, little or no description is obtained, nothing is ascertained concerning the nature of the objects as apprehended. Naming does not necessarily indicate anything in this respect. We may react in the same way to two different objects, yet they may both be particular objects with no generality about them.

In another group of researches the question has been raised as to whether there is a separate 'thought-object' or concept involved when we apprehend an object as generalized. Those who follow Ach and the Würzburg trend (*e.g.* Simoneit[29]) postulate a *Bewusstheit* and a special *Akt* to account for the generality. Fisher, on the other hand, concludes that there is no such 'thought-element' involved, and that the generality consists in the "behavior in consciousness" of certain sensory and imaginal

[28] K. L. Smoke, An objective study of concept formation, *Psychol. Monog.,* 42, 1931-2, (no. 191), 1-46.

[29] M. Simoneit, Beiträge zur Psychologie des Denkens, *Arch. f. d. ges. Psychol.,* 55, 1926, 137-218.

elements.[20] Both sides agree that generalized objects exist. Controversy concerns alone the question of a special thought-element.

Since we are not interested in analysis into elements but rather in a factual description of objects as we find them, we need not enter into the controversy. We shall contend that the aspect of generality is inherent in the object just as physiognomic properties, factuality, color, shape, and size are. Our reasons are exactly the same as those presented in preceding sections.

Rogers and Neff have both described several kinds of object which are produced when an observer is presented with line-drawings or ink-blot designs.[21] Rogers uses the term 'abstractive' to include what we have called generalized objects. Neff, on the other hand, distinguishes the generalized object from an abstractive object which is apprehended as a 'conventional' or incomplete representation of a well-known object.

We may agree with Neff that the abstractive character of a perceived pattern does not necessarily carry with it any generality. Sketchy or incomplete pattern may still be apprehended as a drawing of a particular object, *e.g.* 'this is a drawing of *the* Eiffel Tower,' 'diagram of *my* house,' and so on. A generalized object is more than an incomplete pattern. It may be actually rich in details and still be generalized. Nevertheless, it is also true that abstractive apprehending is often coupled with the apprehension of a generalized object.

As a result of this relation between abstractive pattern and generality, the two are often confused with one another. In the author's study of the solving of various mathematical problems, the observers were sometimes in doubt as to whether their products could be described as generalized or not, because the objects had certain specific properties and details. The following report derived from that experiment, illustrates this fact:

> They were particular objects, pretty definite, anyway. Don't know whether there was a general reference or not, but there was no question that if it were true for these it would be true for any of them. (Le-I-25B)[22]

This probably can be taken to mean that the observer was actually dealing with a generalized object, but that confusion arose because of the definite details involved: We cannot be sure, however, because there is still a third possibility which was not taken account of at that time. Under a task, particularly in thinking and comprehending, we may 'choose an example' of the general class or topic with which we are dealing. Such an example is not necessarily apprehended as generalized. The generality or surrogative rôle which it plays is functional rather than descriptive or 'phenomenal.' It serves in the total functional course *in place* of the topics themselves. While the organism is dealing with the object, however, it is simply *'this* object.' This functional or contextual aspect of objects is extremely important, but we cannot consider it at length here. Its proper place is with the topics of compre-

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[20] S. C. Fisher, The process of generalizing abstraction and its product the general concept, *Psychol. Monog.*, 21, 1916, (no. 90), 1-212.

[21] A. S. Rogers, An analytical study of visual perception, this JOURNAL, 28, 1917, 519-577; W. S. Neff, Perceiving and symbolizing: An experimental study, *ibid.*, 49, 1937, 376-418.

[22] For a description of the method used in obtaining these reports cf. T. A. Ryan, *op. cit.*, 286-289. The report quoted here was not included in the published version of the research.

hending and thinking themselves rather than in a consideration of intermediate activities, and it is a functional problem rather than a problem of description of product.

The discussion above should serve to show how we have limited our use of the term 'generalized object.' It should also show that the term *generalization* as ordinarily used in psychology includes a number of disparate meanings: generalization of response, generalized objects, and surrogative or exemplary generality, all of which refer to quite distinct problems.

When we turn from the isolated generalized object to groups of objects which are ordered and classified, we discover certain further characteristics of generalized objects themselves and also certain properties of the classes and groups into which they are sorted.

We must first of all distinguish classification from mere spatial and temporal grouping. The latter may, at times, be an aid in classification, but it is not to be identified with it. In spatial or temporal grouping we form larger units by integrating individual objects into a spatial or temporal pattern. In an *ordered* group, on the other hand, the pattern is not essential. Instead, the objects are placed in categories or classes; they are generalized. The group is 'objects of such and such a kind,' rather than 'objects forming a circle.'

As evidence that classification is not merely a matter of abstracting bits of pattern common to a group, we may refer to the research of Meili.[28] Here we find that objects are more often classified according to 'use' or 'relation to a topic,' than they are according to color, size, shape, or other spatial-temporal characteristics. Even deliberate attempts on the part of the experimenter to force classification on the basis of color were unsuccessful because of this tendency. Thus a division into 'photographic apparatus' and 'psychological apparatus' was more satisfactory for the observers than a separation into 'brown' and 'black' objects. That is to say, the observers regard the former division as a true classification, or as approaching more nearly the definition of classification (*Ordnung*) than the latter.

Again we see that generality is not to be confused with abstraction. Of course, if nonsense objects are used, classification on the basis of abstracted quality or pattern may be the only possible one. Since nonsense objects have been used so frequently in experiments of this kind, this may account for the confusion of the two terms in the literature.

As a suggestive list of attributes of objects which are generalized we propose: sex, occupation and social level of human beings, kind of animal, uses to which an object is put, classifications in terms of artistic 'schools' or 'periods,' and the like. Such generalizations are probably more frequent than those in terms of color, shape or size, and are also much more important for understanding and comprehending.

It may be objected that all of these attributes depend in some manner upon

[28] R. Meili, Experimentelle Untersuchungen über das Ordnen von Gegenständen, *Psychol. Forsch.*, 7, 1925, 155-193.

abstracted portions of the spatial-temporal pattern, that they are suggested, 'meant' or 'symbolized' by the spatial and temporal pattern. In reply, we contend that the 'cues' which suggest these attributes are not necessarily apprehended separately, and may not be apprehended at all. That is to say, the spatial attributes of an object from which we are supposed to derive a conclusion concerning the object's use are often actually produced at the same time as the generalized property itself. The organism turns out a unitary object with both spatial-temporal properties and other, intermediate, properties, all of which are there at once.

Furthermore, the 'cue' which is supposed to stand for the intermediate property of the object is not necessarily apprehended. The pattern of the *stimulus* itself may be a governing factor rather than the apprehended pattern. Certain relationships in the stimulus may lead us to perceive 'man,' 'fox,' 'photographic implement,' without perceiving a detailed spatial-temporal pattern at all.

Meili has also pointed out certain characteristics of the classified group as a whole. He finds a tendency on the part of his *Os* to make a division which forms symmetrical and balanced groups. But the symmetry-and-balance is not a spatial-temporal symmetry. Rather it is balanced in terms of the generalized characteristics themselves. Both or all of the groups tend to have positive and comparable attributes. A division into 'red objects' and 'cloth' is unsatisfactory.[84]

These and other facts which Meili presents are not, however, important for our problem. We are more interested in the positive properties of classified groups than we are in the conditions which determine their production.

Further study of orders and classes of objects is especially important in as much as apprehended classes probably stand closely related to the large group of topical objects which we may designate as *generalizing topics*. In the latter kind of product, the organism ceases to deal with the objects *in* the class, and deals with the class as an independent product of comprehending. Just how this is accomplished is a question upon which much light can doubtless be thrown by studies of classifying and ordering.

We mentioned earlier the general assumption that all 'thinking' can be accounted for in terms of generalizing and abstraction. Generalized and classified objects do stand closely related to the generalizing topic of comprehending. There are, however, many other kinds of topics of quite different character, kinds which are usually left quite out of account in the genetic theories of comprehending.

*(5) Causal and dependent relations of objects.* Such relationships are closely related to the dynamic properties mentioned as a sub-heading under physiognomic properties. We are dealing here with another aspect of the same, or at least of a closely related problem. There we were interested primarily in single, more or less 'static,' objects which nevertheless exhibited some property of 'usefulness,' either as a permanent or temporary characteristic. In the present section our interest is in certain properties of *events*. Within an event we find that the organism may ap-

[84] R. Meili, *op. cit.*, 173.

prehend more than a bare temporal progression. One thing *leads into* the next, the movement of one object causes the movement of another, the fire causes the water to boil, and so on. The objects involved in the event acquire dynamic properties here also, but they are properties which are derived from the succession of events.

As an illustration of what is meant by the difference between succession and a dynamic or causally integrated event we may contrast the apprehension of people walking across the campus outside my window under ordinary circumstances with the apprehension of a group scurrying to cover because of a sudden thunder-storm. In the latter case the individuals are not just moving; they are moving *away from the rain,* their movements are *apprehended* as caused by the rain.

There are probably also many border-cases between bare succession and causal sequence. Music, for example, is not always a bare succession of tones. As the musician plays in the key of G, F# is apprehended as leading naturally into the succeeding G. We do not speak of the G being caused by, or brought about by, the F#, but still G is the 'natural' or 'most-fitting' next tone. In a well-known melody, each tone leads naturally in the same way into the next. We speak of the melodic sequence as an integrated event-unit.

Events, then, may be divided into the following categories: (1) bare succession of changes; (2) closely integrated event-units; (3) events involving a cause-and-effect sequence. Except for the research upon the apprehension of movement, events at large have received too little attention in psychological investigation. Even in the case of movement, the event has been limited to certain fixed changes of position which ordinarily are only parts of a total event. We tend to study 'stages-in-the-event' rather than the event as a whole, as an integrated and unitary product.

For this reason there is still more to be accomplished with respect to our causal and dependent relations than there is for any of the first four groups of 'intermediate objects.' Nevertheless, a few preliminary remarks may be made.

We have contended that the physiognomic characters may be directly inherent in the objects themselves and that they are not 'projected' any more than color and shape themselves are projected. All are properties of a single, unitary object or product. In a similar fashion, it seems likely that the causal relationships in an event do not always need to be reasoned out or comprehended as separate products. In the simplest form of causal relationship the relationship is inherent in the *event itself.* It is just as much a part of the event as movement or temporal sequence is.

## Conclusions

It has been the purpose of this paper merely to suggest problems by presenting a preliminary catalogue of intermediate objects, and to show the importance of further investigation of these problems for the better understanding of the 'higher mental activities.' We have therefore taken our evidence from many and varied sources, ranging from everyday example to experimental investigation. It is hoped that more evidence will be available in the future and that this will permit a more systematic and more complete discussion.

It will be observed that the general trend of our suggestions has been in the direction of making integral objects out of what are ordinarily considered as dual. Instead of spatial pattern *plus* an inference, we contend that the facts point toward a single object such as 'man who is pleased,' 'causally integrated event,' 'generalized object,' and so on.

This does not necessarily imply that the functions which produce such objects are also unitary. It is conceivable that two functions might work together to turn out a single product, just as the paper industry and the printer combine to produce the book or the newspaper. In these examples, however, the process is a step-wise affair, and it is such a step-wise process which has been traditionally assumed to account for our intermediate products.

There is evidence, however, to substantiate the assumption that intermediate objects, such as physiognomic objects or generalized objects, are not turned out in two steps, first the spatial-temporal pattern, and then, later, the 'meaning.' Instead, what evidence we have suggests that all of the properties issue at the same time as attributive to a single unit.

If, then, resources other than those involved in ordinary apprehending are required to account for these intermediate objects, these resources must be directly incorporated into or integrated with the apprehensive resources of the organism to form an enriched or extended apprehensive activity. As these new resources become more and more important or outstanding, the activity approaches more and more the nature of comprehending proper.

Such an assumption allows us to postulate a continuous series of activities ranging from the barest kind of apprehending, on the one side, to comprehending in which the apprehensive resources are at a minimum, on the other. Evidence for the assumption must, of course, be largely indirect. We need first of all much more adequate descriptions of these intermediate products, and we also need accurate studies of external and historical factors which govern the productive activities.

# A STUDY OF THINKING IN SENILE DETERIORATION AND SCHIZOPHRENIC DISORGANIZATION

By NORMAN CAMERON, Johns Hopkins Hospital

This is a study of thinking, at the level of verbal logic, in a group of 22 patients who had been hospitalized because they were suffering from senile psychoses. It grew out of an earlier investigation in which the thinking of a group of 25 schizophrenic patients was studied by the same method. As stated in that report, "only those patients who were quite unmistakably schizophrenic, who showed definite scattering at the time of the investigation, and who were able to co-operate, were included in the results."[1] After the results of this study had been analyzed and presented, the following questions arose: Is this curious logical form that we find in schizophrenic language disorganization simply the product of a general deterioration? Would it turn out to be just as characteristic of the other reaction-types, for example in the organic deterioration of the aged? The present study offers an answer to these questions by presenting a direct comparison between the logical performance of these two groups of patients—the disorganized schizophrenics and the deteriorated seniles—both of which had been hospitalized because of clinical conditions generally assumed to mean deterioration.

*Subjects.* The senile patients (22 in number), like the schizophrenics with which they are to be compared, also represent a selected group. They had to be able to coöperate with the experimenter in the task set them; and they had to give evidence of the 'organic reaction-type' defect. All showed the kind of gross deficit usually associated with senile deterioration. Only three could give their age in years approximately, the others making errors that ranged from 5 to 50 yr.; six of them placed their ages between 16 and 35 yr. Twenty of the group could not say how long they had been in the hospital; fourteen did not know that it was a hospital at all. Some could not remember their marriage; others gave their children's age as greater than their own or said, for example, that they had children over twenty who were born "three years ago." One who gave her age as "forty" said a minute later that she married at "fifty;" another who said she was "sixty-five" gave her mother's age as "forty-five." The President of the United States was given variously as Coolidge, Theodore Roosevelt, Cleveland, McKinley, or 'Warfield.' Their immediate recall of spoken digits varied from a maximum of 6 forward and 3 reversed to a minimum

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[1] Norman Cameron, Reasoning, regression and communication in schizophrenics, *Psychol., Monog.*, 50, 1938, (no. 221), 1-34.

of 4 forward and none reversed. Ability to subtract serial sevens in all cases was practically absent, only one making even a fair showing. In the face of these facts, it cannot be doubted that our senile material represents the production of clinically deteriorated persons, or that this deterioration is largely conditioned by organic degeneration of the central nervous system.

## Method and Procedure

The method and procedure followed was the same as that used in our schizophrenic study.[3] Sentence-fragments, fifteen in all, were presented orally to the patient in such a way as to call for completion in the form of a causal sequence, for example:

(1) I am in the hospital because....
(2) I am a man (woman) because....
(3) A man fell down in the road because....
(4) A fish can live in the water because....

Every patient was tested individually by the writer. The responses, any auxiliary questions asked, and the replies and comments of the patients were recorded verbatim in shorthand.

## Preliminary Experiments

*Normal children.* Before presenting our comparative results with the schizophrenic and the senile patients, we shall give data we have collected from 29 normal children between the ages of 7 yr. 2 mo. and 11 yr. 5 mo., and from 20 normal adults. In this way we shall be able to confine our comparisons and the conclusions we draw to our own observations and experimental results, and make them quite independent of the outcome of current controversies over the logical studies in childhood reported by others.

The children, who were quite normally alert and coöperative, were patients in the general pediatrics division. No defectives, no neurological cases, and no cases with febrile, infectious or toxic states were included. In Tables I–IV are presented all the responses given to four of the sentence-fragments by these children. In Table I the sex and age of each child is included in the data; in the other tables the same numbers and order of presentation are maintained.

The four sentence-fragments selected represent for the child a diversity of propositions. The first (I am in the hospital because . . .) calls for some explanation of what, to the normal person, would be an indisputable fact with an easy explanation. The second (I am a boy (or girl) because . . .) deals with another indisputable fact, but one that allows greater individual latitude in handling. The third (A man fell down in the road because . . .) presents a simple hypothetical situation, well within the range of everyday life, but for which a variety of causal factors may be invented by the subject. The fourth (A fish can live in the water because . . .) calls for an explanation of the kind of natural phenomenon that is usually taken for granted, without explanation. The first two are personal, the third one other-personal, and the last one quite impersonal.

[3] *Op. cit.*, 6 f.

## TABLE I

### Children's Responses to the First Sentence-Fragment

(1) I am in the hospital because. . . .

| *S* | *Sex and age* | *Response* | *S* | *Sex and age* | *Response* |
|---|---|---|---|---|---|
| 39 | Boy, 7 yr., 2 mo. | "Because I have a broken leg." | 4 | G. 10·0 | "They think there's something wrong with my kidneys." |
| 35 | B. 7·4 | "Sick." | | | |
| 21 | Girl, 7·4 | "Because I was sick." | 17 | G. 10·1 | "Because I'm sick." |
| 40 | B. 7·6 | "Because I was sick." | 36 | B. 10·2 | "Because I got sick." |
| 13 | G. 7·6 | "Because I'm sick." | 18 | G. 10·4 | "My heart is weak." |
| 11 | B. 7·11 | "Sick." | 7 | B. 10·6 | "Because I am sick." |
| 41 | B. 8·4 | "Because I like it." | 33 | B. 10·8 | "I got chorea." |
| 12 | B. 8·4 | "Because I am." | 16 | G. 10·9 | "Because I'm not feeling good." |
| 5 | G. 8·7 | "Because I'm sick." | | | |
| 6 | B. 9·0 | "Because I'm sick." | 8 | B. 10·11 | "I reckon because I got macrocytic anemia." |
| 43 | G. 9·2 | "Because I'm sick." | | | |
| 1 | B. 9·5 | "Because I'm ill." | | | |
| 38 | B. 9·8 | "Because I'm sick." | 19 | G. 11·0 | "Because I am sick." |
| 42 | B. 9·9 | "Because I wanted to see the table" (visitor). | 20 | G. 11·1 | "Because of my eyes." |
| | | | 2 | G. 11·4 | "Because I'm sick." |
| | | | 3 | G. 11·4 | "Because I'm sick." |
| 37 | B. 9·9 | "Because I'm sick." | 23 | G. 11·5 | "Because I have disease of the heart." |
| 44 | B. 9·11 | "Because I am sick." | | | |

## TABLE II

### Children's Responses to the Second Sentence-Fragment

(2) I am a boy (girl) because. . . .

Case 39, "Because God made me;" 35, "Because God made me a boy;" 21, "Because the Lord made me;" 40, "I was born that way, a boy;" 13, "Because he's a boy" [What makes you a girl?] "I don't know;" 11, "I was born that way;" 41, "Because I like it;" 12, "Because I am;" 5, "That one's hard. I don't know that one;" 6, "I growed a boy;" 43, "Because when I was born I was a boy;" 1, "Because I wear pants;" 38, "Because I was born that way;" 42, "Because I was born a boy;" 37, " I don't know;" 44, "Because I was born a boy;" 4, "Because I was born to be a girl;" 17, "I was made that way, I guess;" 36, "I was born that way;" 18, "Because I wear dresses;" 7, "I was born a boy;" 33, "Because I was born;" 16, "Because you're born for a girl;" 8, "Because I'm young;" 19, "Because I was born a girl;" 20, "I was born that way;" 2, "Because I was born like a girl. That's a funny question;" 3, "Because I am." [Why are you?] "Because I was a girl when I was born;" 23, "God made me one."

## TABLE III

### Children's Responses to the Third Sentence-Fragment

(3) A man fell down in the road because. . . .

Case 38, "It was a brick, or rather a rock; he stumbled over it;" 35, "He slid down in the road;" 21, "I was sorry;" 40, "Because he stumbled;" 13, "Because he had a sore leg;" 11, "Because he was riding on a horse;" 41, "We helped him;" 12, "Because he did;" 5, "Because he stubbed his toe;" 6, "Because he stumbled;" 43, "Automobile hit him;" 1, "Because he stumbled;" 38, "He was hit;" 42, "Because he slipped—or a car;" 37, "He was crippled;" 44, "Because he was drunk;" 4, "Maybe he wasn't looking where he was going;" 17, "He didn't look where he was going;" 36, "Because he slipped;" 18, "Because he was careless;" 7, "The road was icy;" 33, "Because he stumbled;" 16, "There was ice;" 8, "Because he was dizzy;" 19, "He got hit by a car;" 20, "Because he was drunk;" 2, "Because he was drunk;" 3, "He stubbed his toe;" 23, "Because he was weak."

TABLE IV

CHILDREN'S RESPONSES TO THE FOURTH SENTENCE-FRAGMENT

(4) A fish can live in the water because. . . .

Case 39, "Because it has fins or a tail;" 35, "Because it can swim;" 21, "Because they like the water;" 40, "Because he drinks the water;" 13, "Because the water's warm." [Can a fish live in cold water?] "No." [What about the fish that live in the ocean?] "They can live in cold and warm water;" 11, "Because he can live better;" 41, "Because it likes to swim;" 12, "Because he can;" 5, "Because he can't live out of the water." [How can he live in it?] "Because if he didn't live in it he'd die; he has to live in it;" 6, "Because it breathes the water;" 43, "It will die on land;" 1, "Because he can't breathe on land;" 38, "I don't know;" 42, "Because he can't live out of the water." [Why can he live in it?] "I don't know. We can't but he can. It's just like to him, like air is to us. If he got out, the air would be like the water is to us;" 37, "Because he can swim;" 44, "Because it was made to live in the water;" 4, "Because it was born in the water." [Could you live in the water if you were born in it?] "Yes;" 17, "It's the only way he can live; he can't live out of water;" 36, "Because it is used to it." [Can he live out of it?] "No." [Why?] "He can't walk very good;" 18, "Because it likes the water;" 7, "Because he lives there;" 33, "He can't breathe at night." [Can he breathe in the day?] "Sure he can breathe in the day or night;" 16, "Because he can't walk, and he can't do nothing else he has to;" 8, "Because it can breathe under water;" 19, "It likes the water;" 20, "Because it was born in the water;" 2, "Because he lives in the water." [How can he live in it?] "Because we can't live in it and he can." [How can he?] "Because he swims in the water." [How can he *live* in the water?] "Because he's a fish;" 3, "Because it stands the water and we don't." [Why?] "Because it was born in the water;" 23, "He is supposed to; he can't live on land."

*Normal adults.* By way of indicating the logical performance of the normal person, as a farther basis for comparison, we show in Tables V–VIII all the responses given by a group of 20 adults who were not hospitalized at all.

These *Ss* were hospital attendants, stenographers, and two nurses without college training. Thus the educational and social backgrounds are roughly the same as those of our senile and schizophrenic patients, since among our patients were persons with grade school and high school education, and one with three years in a well-known college. The arrangement of the material in the following tables is the same as that in the preceding ones.

TABLE V

NORMAL ADULTS' RESPONSES TO THE FIRST SENTENCE-FRAGMENT

(1) I am in the hospital because. . . .

Case 51, "Because I'm employed here;" 52, "Because I work here;" 53, "I'm very much interested in this work; of course, I'm making my living;" 54, "Because I am working here;" 55, "Because I like nursing;" 56, "Because I'm ill;" 57, "I work here;" 58, "Because I work here, I guess;" 59, "Because I could not get another job;" 60, "Because of lack of employment on the outside;" 61, "Because I'm working here;" 62, "Because I'm helping mentally deficient patients;" 63, "I work here;" 64, "Because I like to work;" 65, "Because I work here;" 66, "Because of the medical treatment I can receive;" 67, "Because I'm an attendant, and to perform my daily duties;" 68, "To help patients;" 69, "Because I'm interested in this type of work;" 70, "I wish to work."

## TABLE VI

### Normal Adults' Responses to the Second Sentence-Fragment

(2) I am a man (woman) because. . . .

Case 51, "Because of the physical difference; I'm a girl because I wear a dress;" 52, "Because I was born that way;" 53, "I was born a girl;" 54, "Because I am not a man;" 55, "Because I was born that way;" 56, "That's the way I happened to be;" 57, "I was born a male;" 58, "Because I'm not a girl;" 59, "I was born that way;" 60, "Because I couldn't be a female;" 61, "Because I was born a female;" 62, "Because I'm not a male;" 63, "Because I was born a female;" 64, "Because that was the way I was born;" 65, "Because I was born that way;" 66, "I'm of age;" 67, "Because I'm not a male;" 68, "I was born so;" 69, "Because God created me so;" 70, "I was born a woman."

## TABLE VII

### Normal Adults' Responses to the Third Sentence-Fragment

(3) A man fell down in the road because. . . .

Case 51, "Because he stumbled." [Did he stumble first or fall first?] "He stumbled first;" 52, "Because he couldn't keep his balance." [Why couldn't he?] "Because he slipped;" 53, "Because he could not stand up." [Why couldn't he stand up?] "He might have been ill or something might have tripped him up;" 54, "Because there was a rope across that he didn't see;" 55, "The weather was so warm" (heat wave); 56, "Because he was careless;" 57, "Because he tripped;" 58, "He couldn't stand up." [Why couldn't he stand up?] "Because he slipped;" 59, "Because he slipped;" 60, "Because he couldn't prevent it;" 61, "Because he stumbled on a rock;" 62, "Because he turned his ankle;" 63, "Because he stubbed his toe;" 64, "He couldn't stand up;" 65, "Because he slipped;" 66, "Because he stumbled over something;" 67, "Because he stumbled;" 68, "Because he stumbled, or he couldn't help himself; 69, "Because he stumbled;" 70, "Because he stubbed his toe."

## TABLE VIII

### Normal Adults' Responses to the Fourth Sentence-Fragment

(4) A fish can live in the water because. . . .

Case 51, "They have the power to breathe while under water;" 52, "Because it has gills and is equipped to breathe—as we breathe air through our lungs;" 53, "Because it cannot live on land;" 54, "He gets oxygen out of the water;" 55, "That is its environment;" 56, "Because it can breathe under water;" 57, "A fish comes up for oxygen once in a while;" 58, "It's nature for him to live in the water." [How is he able to do it?] "That's how he lives—breathes water instead of air;" 59, "Because he has no lungs." [How is he able to live in the water?] "He breathes through his gills;" 60, "Because it could not live on land;" 61, "Because he can come up to the surface and get enough air to keep him swimming for a while, but I don't know for how long;" 62, "Because it doesn't breathe oxygen. It has to breathe under water." [What does it breathe?] "It doesn't breathe air;" 63, "Because they have their food to live in the water, and they couldn't live on earth;" 64, "Because they have no lungs, do they?" [How can they live in the water?] "They're the type of animal that survive in the water. Their nourishment comes from the water;" 65, "Because it's built to be able to live under the water." [What do you mean?] "Built so he can breathe under water;" 66, "Because it has gills for the purpose;" 67, "Because it cannot live on land; they don't need no oxygen;" 68, "Because there's no other way for it to live." [How is it able to live in the water?] "By swimming;" 69, "It's the nature of the fish." [How is he able to?] "He must be able to breathe under the water." [How can he?] "It's the way he's constructed. He has facilities for breathing that we don't have;" 70, "Because it can swim." [Could he if he couldn't swim?] "No, because he has to get his food."

## Main Experiments

*Senile and schizophrenic patients.* In our comparison of the so-called 'deteriorations' we shall be interested primarily in seeing to what extent the peculiar characteristics which we have already emphasized in connection with schizophrenic patients occur to a comparable degree in cases of known organic deterioration. In schizophrenic language disorganization we have described, in the first place, a form in which loose clusters of terms are given by the patient instead of the more restricted and organized conceptual structures. He fails to provide adequate explicit connecting links between the elements of these cluster-forms and so cannot eliminate the incidental and the partially relevant constituents sufficiently to permit a close organization. Even more strikingly than this, he tends to give in place of the more precise definitive term or phrase of the normal adult, an approximate substitute term or phrase. This often results in the use of a personal idiom that may become so highly individualistic as to be useless as an instrument of social communication. We have given numerous illustrations of both these forms as we found them in our own schizophrenics;[3] and in the study of language protocols in schizophrenia by Woods, that has recently appeared, other examples can be found.[4]

We shall present in Tables IX–XVI all of the responses given to the same sentence-fragments by our senile and schizophrenic groups. The arrangement of data is as follows. The responses listed together in each table under "A" are quite comparable with the normal results; while the "B" group in each table gives the more pathological material. These responses are arranged in descending order, with the questionable or slightly unusual ones near the top, and the more definitely pathological ones near the bottom. The case numbers are given to bring out the spread, *i.e.* most of the individuals have responses that are listed in both Sections, A and B.

If Tables IX and X be compared, the differences between the groups will become obvious. The twelve senile responses in the Section B of Table IX show in eight cases an account that is plausible, even if not correct, of injury, illness, ordinary disability, or of misidentification of the hospital as their usual place of work (Cases 101, 107, 110, 115, 116, 118, 119, 120). These were also expressed in acceptable form; but the statements were for the most part inventions that do not tally with the

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[3] *Op. cit.*, 14-27.

[4] William Woods, Language study in schizophrenia, *J. Nerv. & Ment. Dis.*, 87, 1938, 290-316.

actual reasons for hospitalization. There is one loose cluster-form present (Case 121).

TABLE IX

SENILES' RESPONSES TO THE FIRST SENTENCE-FRAGMENT

(1) I am in the hospital because. . . .

(A) Case 106, "Because I'm sick;" 105, "Because I'm not feeling well;" 104, "Because of nervousness;" 114, "Because I'm so nervous and my husband is in the state hospital;" 122, "Because I had no other place to go;" 109, "Because my mother, father and sister died in one year. I didn't have no place to go;" 108, "Because my husband took sick." [Why are *you* in the hospital?] "On account of him. When he took sick, they took me here, and I've been here ever since;" 103, "Because my brother thought it was the place for me—if it *is* a hospital. It's better to call it a 'home';" 112, "Because I wasn't able to work."

(B) Case 101, "Because I hurt myself;" 118, "Because I fell down just before I came here;" 107, "Because of my condition." [What is the condition?] "I'm getting a lot better and I feel a lot better." [Why did you come in the first place?] "Run down—the hot summer;" 116, "Because my feet were so bad and they had the hot heat on them" (lamp); 115, "Because I couldn't wear the glasses no longer;" 113, "I don't know;" 111, "Because I'm satisfied;" 121, "Because I'm not able, or sick or something like that;" 110, "Because my husband died and I've been working for 13 years here" [Patient thinks this is the state laundry, and that she is an employee of it]; 120, "I just stepped in this morning to see if Miss Quigley wants me;" 119, "I'm not in a hospital. I'm here because I came on a visit;" 117, (No answer.)

TABLE X

SCHIZOPHRENICS' RESPONSES TO THE FIRST SENTENCE-FRAGMENT

(1) I am in the hospital because. . . .

(A) Case 11, "Because I'm sick;" 23, "Because I am sick;" 15, "Because I was sick;" 3, "Because of sickness;" 24, "Because I'm not well;" 7, "Because I had a nervous breakdown on the 28th of February, 1934."

(B) Case 4, "Because I wasn't working. I'll be 28 in October;" 19, "Because I am here;" 13, "Because I lost a job and wasn't working;" 6, "I am physically unable to be in a private home" (not true); 22, "Because I tried to kill my mother;" 17, "He is a patient" (No one else present); 2, "I was entered in here three different times under the same name;" 9, "I am in Church Infirmary, D ward for labor;" 8, "I work here for the United States Government;" 10, "Because of sex weakness;" 5, "Because I love life;" 18, "Because I was in love with a sister;" 12, "Because of Egyptian methods, I think;" 20, "Because it is my own property and it is God's will;" 14, "Because I won't take a key away from an attendant, and because I'm the man that makes the key;" 25, "I'm just mixed up. That fellow wants to marry me; he thinks I'm a woman" (a man responding); 16, "Because I was transferred due to work over here due to methodical change of environment, temporary;" 1, "Because of investigation of a charge and understanding of it as a condition according to Almighty God on account of a child that could not have anything on account of a constant change and abuse;" 21, "Because I was hatched." [Why hatched?] "To separate chains from it; the papers will be there."

In contrast to this, the schizophrenic responses in the Section B of Table X deviate farther and farther as one goes down the list. The first response

shows juxtaposition. Eight of the next thirteen reveal traces of their delusional material in the reasons they give for their presence in the hospital (Cases 2, 6, 8, 9, 10, 12, 18, 20). The last three represent definite distortions of form and material, for which there is nothing comparable in the corresponding senile group. The distorted reply given by Case 16, for instance, referred to an actual fact—he had been transferred from another building in the same hospital; but the form of the statement is vague and disorganized. It would have to be translated into something like this, "I was transferred to this building, temporarily, because there is work over here to be done, and it provides a change of environment for me." Without knowledge of some of the actual facts in his situation, of course, such a translation would be a mere guess, since these patients are very often unable to clarify their vague approximations farther upon demand. Similarly, Case 1 is accounting in a typical manner for his presence in the hospital by alluding to his sexual misconceptions. All of the last six responses bring out a characteristically loose cluster-form of structure that appears almost exclusively in the schizophrenic tables.

The next comparison, between Tables XI and XII, deals also with a personal theme. Part of the superiority of the senile group, which placed twelve answers in the normal Section A, can be ascribed to their preference for deistic forms of explanation which were more in vogue among adults during their earlier lives than now. Of the nine senile responses placed in the Section B, four relate their sex to their work (Cases 105, 110, 112, 118), two to their age (115, 116), and one refers vaguely to heredity (111).

In the comparable schizophrenic material of Table XII, the last nine responses deserve special attention (Case 22 to the end). Case 22 throws in three loosely related terms, "It isn't a he" for "I'm not a 'he'" and then gratuitously adds the "me" to an otherwise intelligible statement. Case 16 describes by typical schizophrenic indirection the patient's gropings for perspective in life. Case 14 is perpetually preoccupied by a battle between God and the devil in which his own rôle shifts from one side to the other. The brief response of Case 17 shows a distorted trace of his college experience. Finally, Case 13 produces in writing a piece of verse that is full of transformations and neologistic approximations. There are really no equivalents of these responses in the senile material.

In Tables XIII and XIV are listed the responses to a simple hypothetical situation of ordinary experience, the fall of another person. Of the twenty-two seniles, eleven gave plausible interpretations in terms of alcoholism (Cases 103, 113, 117, 120) or of stumbling, injury, illness or

TABLE XI

SENILES' RESPONSES TO THE SECOND SENTENCE-FRAGMENT

(2) I am a man (woman) because. . . .

(A) Case 101, "Because I was made one;" 119, "Because I was born that way;" 122, "Because I was born that way;" 102, "Because God made me a woman;" 104, "Because God created me a woman;" 103, "Because that's the way the good Lord made me;" 106, "Because the Lord made me that way;" 113, "Because God wanted it to be so;" 107, "Because I am a female;" 109, "Because I couldn't be a man;" 108, "Because I ain't no man."

(B) Case 117, "Born, I guess. I don't know;" 105, "Because the Lord wanted me to be a woman. Of course I always have a good deal of work to do. My husband can do anything he wants to;" 118, "Because I used to go to work;" 110, "Because I do a woman's work," 112, "Because I must do as a lady does," 111, "My father and mother bring me here." [How does that make you a woman?] "Because my mother brought me into the world;" 115, "I'm an old woman." [Why are you a woman?] "Because my children is all grown. I want my glasses;" 116, "I was a child 'oncet' in my life." [Why are you a woman?] "Because I'm an old woman, and I've been born a good while;" 120, "I couldn't answer that at all. I'll tell you, I don't know how to say it."

TABLE XII

SCHIZOPHRENICS' RESPONSES TO THE SECOND SENTENCE-FRAGMENT

(2) I am a man (woman) because. . . .

(A) Case 1, "Because I was born a man;" 10, "Because I'm a male;" 3, "Because I was born that way;" 6, "Because I'm born so;" 12, "Because I was born that way, feminine way;" 19, "Because I am a woman."

(B) Case 9, "Because born male;" 8, "Because when you get to age you're a man;" 25, "Because I'm not grown up yet;" 23, Because I am a boy. I don't know. I am a boy because. . . . [Because?] "Because I'm. . . ." 24, "Because of my sex;" 11, "I try to be one;" 18, "Because I like to be a lady;" 2, "Because I think it's just better to be a woman; but I'm not strong enough to earn my living;" 4, "Because I do business," 5, "Because I am a girl." [Why?] "I am a woman because I am a woman. There is no other reason, 126, 127, Mr. —— ——;" 22, "I am a girl because I am a she. It isn't a he or a me;" 20, "Because God created *you* a man;" 16, "Trying to find me or way of trying to get a proper vision." [What does that mean?] "Working for an assignment where I can be placed somewhere to be of service." [What assignment would you like?] "The way I feel I am not very capable;" 14, "Because the devil is there;" 15, "Because my grandfather has died;" 21, "Because the books are all scribbled up." [Is that why you are a woman?] "Yes." [Why are you a woman?] "I don't know;" 17, "That's ethnography;" 7, "Because through some sort of a gas cubic disease, forms a quinsy disease." [I am a man because. . . .] "I was born a male sense;" 13, "Lad, die of Scotland, Yard of Newgate Sea, Drivin, not censer flames, Pompey Emerald bea, Circulivin nautical intervengeance draught Sider lea. [Written by the patient. He says "Lad, die" means "lady." Much homosexual material in his talk.]

weakness (102, 103, 104, 107, 110, 114, 119, 121). Bleuler has emphasized the tendency among seniles, even while their general comprehension is still good, to supply personal information when it is not called for by a question.[5] Two of our more deteriorated seniles gave data con-

[5] P. E. Bleuler, *Lehrbuch der Psychiatrie*, 5th ed., 1930, 49.

cerning their own mishaps that was quite out of place, although formally correct (101, 115), and three others interpreted the situation as a real one (106, 112, 121). The last response listed is in a class by itself and reads more like one of the moderately scattered schizophrenics.

TABLE XIII

SENILES' RESPONSES TO THE THIRD SENTENCE-FRAGMENT

(3) A man fell down in the road because. . . .

(A) Case 114, "Because he tripped;" 110, "Because he stumbled; he fell when he stumbled;" 104, "Because he stumbled." [Did he stumble first or fall first?] "He stumbled before he fell;" 102, "Because he got hurt." [Did he fall down before he got hurt?] "I shouldn't think so. Maybe he was weak;" 103, "Because he stubbed his toe; otherwise he might have been drinking;" 117, "Because he was drunk, I guess;" 119, "Because he was sick, I suppose. I don't know;" 105, "Because he couldn't stand up;" 116, "Because he couldn't stand up, I reckon."

(B) Case 120, "Because he wasn't able to keep on his feet—too much 'smile' (Goes on to tell a circumstantial story about alcoholics who have taken too much 'smile'); 108, "Because he wasn't able to walk. It's like me." (Patient unable to walk); 112, "He may be sick;" 109, "Because he fell, or a stone hit him;" 113, "Because he wanted to; or else he was drunk;" 111, "Because he was crazy" (laughs); 101, "Because he fell." [Why did he fall?] "You say he had no disease, don't you? I fell against a tree, I was so giddy." 121, "Because he hurt himself." [Did he fall down first?] "I didn't see him at all. I didn't know there was something like that going on;" 115, "I fell down when I stumbled. I stumbled over my toe when I was nine or ten;" 106, "Because he didn't have sense enough to stand." [Why should he fall?] "He thought he didn't have any chance. He wasn't hurt;" 107, "Because he couldn't cross it." [Why couldn't he cross it?] "Because he couldn't cross it, is the answer. He might have fell and hurt himself so that he couldn't;" 122, "Because I think he might steal, or he might be good;" 118, (no answer).

TABLE XIV

SCHIZOPHRENICS' RESPONSES TO THE THIRD SENTENCE-FRAGMENT

(3) A man fell down in the road because. . . .

(A) Case 10, "Because he stumbled;" 19, "Because he slipped;" 22, "Because he hurt himself." [Did he fall down because he hurt himself, or did he hurt himself because he fell down?] "He fell down because he hurt himself, yeah;" 9, "Because he met with an accident;" 20, "He may have been knocked down;" 8, "He must have been weak;" 11, "Because he couldn't help it."

(B) Case 3, "Because he got hurt." [Did he fall down first?] "He got hurt when he fell;" 6, "Because he broke his toe;" 4, "Because he stumbled or else fell." [Did he stumble and then fall?] "I don't think he stumbled;" 16, "Because of accident;" 18, "Carelessness, I think;" 1, "Because it was an accident or because it is an accident;" 5, "Because he fell down." [Why did he?] "He fell down and went boom! [Why?] "Because he fell down;" 24. "Because he stumbled or careless. I don't know," 15, "Because he was injured and shot;" 25, "Because he probably got electrocuted and shot and was probably dead;" 2, "On account of me;" 17, "Because I slipped," 23, "Because of the World War;" 12, "Because of a magnet in Holland;" 21, "Because he was hatched and then the world turned over," 13, "A man fell down in the road because, Thinking it was shame in clause, Sentence, meaning richness, shoes or peers, Not Iolanthe, queer Fatima, molten lava, leers" (Written down by patient); 7, "Because he might have had full conscience depressed if overcrowded area; kidney disease of some kind;" 14, (no answer).

The schizophrenic material in Table XIV again offers a distinct contrast, not only in the actual logic but in the form of the statements. Quite striking is the occurrence of a peculiar organization in which two or three elements are merely thrown in together, in a place where only one element is really needed ("injured and shot," "stumbled or careless," "electrocuted and shot and was probably dead," "hatched and the world turned over," "stumbled or else fell," and the less quotable vagaries of Cases 7 and 13). One considers herself the cause of the fall, another attributes it to the World War (never a soldier), and another to "a magnet in Holland." The loose cluster-form of the more disorganized answers, and the extreme vagueness of reference in so many, have virtually nothing in common with the great majority of our senile responses.

Finally, we consider the natural history problem in Tables XV and XVI. If anything, the schizophrenics as the younger group should have the advantage; and the more normal responses in the Section A are really quite comparable. (The similarity between Cases 22 and 122 is merely an interesting coincidence.) Among the seniles, we see some tendency to

TABLE XV

SENILES' RESPONSES TO THE FOURTH SENTENCE-FRAGMENT

(4) A fish can live in the water because. . . .

(A) Case 116, "Because he can't live out of it;" 104, "Because he can't live any place else;" 102, "Because the Lord sent him there in the water;" 113, "That's what it's here for;" 119, "Because they're born in it. It's natural;" 108, "Because the water's nice for him. When he gets out of the water he dies;" 105, "Because it swims." [If it didn't swim could it?] "I don't know."

(B) Case 122, "Because of being a natural water-bug;" 120, "I suppose it's natural to them; and they'll live in there as long as you'll let them. Maw doesn't care for fish;" 114, "Because he's used to the water. Because the water's always around him and he's crazy for the water. I like fish;" 110, "Because he's used to it." [How can he live in it at first?] "Until they get used to it they lay down on the bottom;" 111, "Because he likes the water. He likes to swim;" 117, "Because they live in the water. They can swim; they get along all right;" 107, "Because he swims and he has gills to swim with." [Does he swim with gills?] "Yes, partly." [How does he breathe?] "Through his mouth." [Does he have lungs?] "Oh, yes, same as a person;" 118, "Because it wants to." [How is it able to?] "It swims with its feet—those wide." [Fins?] "Yes;" 112, "Because it's living." [How can it live in the water?] "It feeds in the water;" 103, "That's the way it was made to do." [But how is it able to?] "It's the fins." [What does it do with the fins?] "It moves them back and forth and sidewise;" 101, "Because he was made to be, or die in the water. They die in the water, too;" 109, "Because I want to eat them and get them out." [How can they live in the water?] "They can live because they're always in the water. Oh, I'm a pretty good scholar;" 121, "It's something about his going around in the water;" 106, "Because he likes it. He can find them pretty good if he goes at it." [What?] "Anything the Lords wants him to have." [How can a fish live in the water?] "If they let it alone, it'll stay pretty long;" 115, "A fish has to swim. He has to get the fish, wash the fish. Everything is the main thing on Sunday there."

TABLE XVI

SCHIZOPHRENICS' RESPONSES TO THE FOURTH SENTENCE-FRAGMENT

(4) A fish can live in the water because. . . .

(A) Case 8, "Because they can't live on dry land;" 3, "Because they cannot live on land." [How can they live under water?] "Because he is born in the water;" 18, "Because it wouldn't live if it were not in the water. Because they need water;" 1, "It can't live out of it." [Why can it live in it?] "Natural law;" 11, "Because he is made that way;" 12, "Because it is made that way and there's oxygen in the water;" 10, "Because it is used to it. It needs water;" 9, "Because it swims." [And if it doesn't swim?] "I don't know."

(B) Case 22, "Because it is a water-bug;" 24, "Because of sea-food;" 16, "Because of breathing through their fins." [Their fins?] "Gills;" 5, "He can live in the water because he can live in the water." [Why?] "We cannot live in the water because we cannot live in the water;" 23, "That's easy enough. Why does a fish live in the water?" [Question repeated] "He just does;" 19, "Because it swims." [If it couldn't swim, could it live in the water?] "Yes, it would be like an oyster;" 17, "Because they could swim." [If they couldn't swim, could they live in the water?] "Yes, the flying fishes;" 7, "Because it's the natural resource of life;" 15, "Because they breathe under air, water or something;" 20, "His length and the way he breathes and his gills;" 2, "Because they want to get under-seas to do what other things do;" 21, "Because they only eat them." [Why can they live in the water?] "Because it's just the circumstances. Our Lord's wonderful supply is the vegetation;" 13, "Because having no eyes to see on land if it crawls," 14, "A fish is not able to live in the water." [But they do.] They *do* just simply because of the word *breath*, and that is a fact of there being God;" 6, "Because it has learned to swim." [What if it could not swim?] "Not naturally, he couldn't. Why do certain gods have effects on seas like that? What does the earth have such an effect to break their backs? The fishes near home come to the surface and break." [Why?] "I think it is due to bodies that people lose. A body becomes adapted to the air. Think thoughts and break the fishes;" 4, "Because it was soft." [What do you mean by that?] "It must have a soft bottom." [The fish or the water?] "What?" [Can a fish live if the water has a hard bottom?] "If it was a catfish, I think it could, because I think they are alive when they come out of the water;" 25, "Because it can't swim." [It *can't* swim?] "It's dry." [What do you mean, 'dry'?] "When it's dry, the wet would like to get with the dry." [Yes?] "And it gets wet. It ought to get out of the water and quit swimming around."

impute to the fish pleasure and preference as to his aquatic habitat (Cases 106, 108, 111, 114, 118), a few more or less irrelevant comments on fish as food (109, 114, 115, 120), and simple misstatements that can be safely ascribed to ignorance (107, 110, 118). Some of these natural science responses of our seniles do, however, show more vagueness and looseness of logical structure than we have been finding in the preceding more personal or more practical subjects. The last six answers are interesting. Case 121 is exceptionally vague. Each of the other five has something of no causal significance included (direction in which the fins are moved, dying of the fish, eating the fish, leaving them alone, washing fish for Sunday). Some are not unlike a type of logic that Huang obtained in a similar problem, working with college girls, whenever they lacked the information

necessary to explain certain other natural phenomena about which he asked.[6]

Turning, now, however, to Table XVI for comparison, one is impressed again with the difference in form and material. A brief inspection of all the eleven responses following Case 19 will bring out this contrast better than any description will. There was no trace of elation in Case 17's second answer to help account for it. The phrase given by Case 7 offers an example of the schizophrenic type of approximation; while Cases 15 and 20 show clearly the cluster-form with its lack of explicit organization. Case 21 is saying, in her own idiom, that fish are able to live in the water because the Lord supplies food for them. Case 13 pictures vaguely a disabled creature out of its element; and Case 14, after a contradiction, meets the problem in his idiom by bringing in "the word," "breath," and "God," in a loose cluster-organization. The last three cases are least of all capable of being compared with anything in the senile productions.

## Discussion

It is not without general significance that our senile group, in spite of devastating memory deficits and hopeless disorientation, in its use of the instruments of communication was definitely superior to our schizophrenics, who knew where they were, who the experimenter was, what the year and season were, and when and where they were born. To be understood adequately, language must be considered as preëminently a social process. Elsewhere we have discussed the rôle of social impact in the development of the child's symbols;[7] and we have given examples of arrested development appearing in the normal adult when he attempts to utilize a neglected form of symbolization.[8] Here we are concerned with processes involving reduction in the effectiveness of logical structures, in one group as a part of schizophrenic disorganization and in the other as an outcome of senile dilapidation.

It is a striking fact that many senile psychotics show a remarkable preservation of general social function up to an advanced stage of deterioration. Thus one may find observance of the simpler social amenities, of courtesy, modesty, considerateness and deference, even in a patient who is hopelessly lost, lives in the last century, waits for long-dead parents to take her to a home that no longer exists, and forgets what she has been

[6] I. Huang, Children's explanations of strange phenomena, *Psychol. Forsch.*, 14, 1930, 63-182.

[7] Cameron, Individual and social factors in the development of graphic symbolization, *J. Psychol.*, 5, 1938, 165-184.

[8] Cameron, Functional immaturity in the symbolization of scientifically trained adults, *ibid.*, 6, 1938, 161-175.

told a few moments before. Now among the most important of all the social forms is the system of language responses that one has used during most of a lifetime. In the absence of specific focal lesions, even a deteriorated senile patient may still have at his disposal a variety of social forms in the verbal field capable of functioning appropriately.[9] These do not, of course, entirely escape the general narrowing down of the field of potential activity, but such a restriction in itself may be to some extent actually helpful, since the limitation of available material simplifies the problem of keeping it organized. This is only a special instance of our more general clinical experience, that acute senile confusion can be improved markedly when one radically simplifies the environment by restricting the variety of situations with which the senile needs to cope.

The form of the responses and their adequacy as tools of social communication are related to the more general question of attitude. The seniles as a group were much more given to self-criticism in relation to their performance of the task. Many of them expressed uncertainty and dissatisfaction with their results, by remarking on their own inadequacy, or by trying to explain away or laugh away their short-comings. Some of them showed an expectation of corroboration or of correction from the experimenter, thereby indicating, to a much greater degree than did the schizophrenics, their social capacity for anticipating or adopting the attitude of the other person. In five members of our senile group there was acknowledgement of intellectual deficit on their part, which in two was experienced as a painful realization. In this we can agree with Jelgersma that the opposite has been too much stressed.[10] He found in the course of his own investigations, as we did, "senile dements in a quite far advanced stage of the illness who related that their memory was bad, much worse than before."[11]

The common attitude of evasiveness among our seniles gave us the impression that it was serving as a defensive screen to help conceal these deficits. By contrast, our disorganized schizophrenics seemed to miss rather than to evade points. Indeed, often they offered two or three pseudo-equivalent terms in places where one term alone would have done better. They were often strikingly undisturbed or well-satisfied with statements that were slip-shod, incomplete, jumbled, and even quite unintelligible. Their further explanation was often less clear than the original statement it was meant to qualify; instead of restricting the reference by such

[9] W. Spielmeyer, Die Psychosen des Rückbildungs- und Greisenalters, in Aschaffenburg's *Handbuch der Psychiatrie*, 1912, 49.
[10] H. C. Jelgersma, Die Psychoanalyse der Dementia senilis, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 135, 1931, 657-670.
[11] *Ibid.*, 664.

qualification, they increased the unclearness by adding elements. Their structure included loose cluster-forms, approximate substitute terms and phrases, and the use of highly individual idioms.

It is not surprising that, in these late-products of psychoses, the language structure should differ. In addition to marked differences in the post-mortem pictures of the two groups, we are dealing with the outcome of different histories, different personalities, different processes, and different age-groups. Our schizophrenics for the most part have gradually withdrawn from the business of life; they have 'retired' long before their time. Most of them have been very unsuccessful in maintaining effective social relationships with the persons and things about them. Our seniles, on the contrary, have been successful in maintaining reasonably adequate social relationships with their personal and material surroundings throughout most of a long life span. Their deterioration has not been a withdrawal from life's stage; on the contrary, they have been unwillingly pushed from it by the defects of their failing machinery. Some of them even now have to be constantly restricted in the scope of their activities, because they cannot keep within the limits of their present capacity to handle the environment. In our material this irreversible dilapidation of pathologic old-age neither gives us a total picture like that of disorganized schizophrenics nor yields the same language product.

## Summary

*(1)* A comparison of the verbal productions and logical form was made between selected groups of deteriorated seniles and disorganized schizophrenics; and data, gathered by us from normal children and normal adults, were presented in order that our conclusions may be independent of current controversies over the results of the genetic studies of others.

*(2)* In our material, neither senile dilapidation nor schizophrenic disorganization yielded a language product comparable with our own material from normal children.

*(3)* The seniles rarely exhibited either the loose cluster-form of organization (*asyndesis*), or the substitution of inexact approximations for more precise terms and phrases so characteristically found in the schizophrenic material.

*(4)* Finally, the dissimilarities in language structure that occur in these two forms of so-called 'deterioration' are discussed in relation to the more general differences to be found in biographical development, in prepsychotic personality, in the age-period of onset, and in the organic pathology of the two groups.

# THE RELATION BETWEEN GOODNESS OF PERFORMANCE AND FAVORABLENESS OF CONDITIONS

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The chief purpose of the present series of studies is to ascertain the law connecting goodness of performance with changes in the favorableness of its conditions.[1] Goodness of performance, measured in scaled units, constitutes the most general possible psychological variable. It may be used to express almost any outcome of almost any task-solving activity, *e.g.* it may be used to express accuracy of estimation, keenness of comparison, amount done, time required, or number of errors. Instead of attempting to correlate this very general attribute with its innumerable determining conditions considered in detail—an interminable problem the answer to which would constitute a large part of the whole science of psychology—the investigation has centered upon relating goodness of performance to one attribute possessed by all conditions. It has been found feasible to formulate a general attribute of all determining conditions, under which the psychological pertinence of any condition may be subsumed, just as any score may be expressed in terms of scaled goodness. Such a general denominator for the conditions affecting task-accomplishment is afforded by a new dimension described in the second part of this paper, termed simply the *favorableness* of conditions. The procedure for laying off units on this dimension is there described in detail. It is based on the concept of equivalent changes. A law connecting the two variables, goodness of performance and the favorableness of conditions is also formulated.

The experimental investigation described in Section I of this paper not only serves to illustrate the more general problem described above and the method of attack which has been used, but also contributes information, in addition to that obtained in the previous experiments, which is indispensable even to a tentative answer.

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[1] This is the sixth of a series of investigations concerning the relation of the goodness of a performance to the total constellation of its conditions. For preceding studies, see the following: The measurement of difficulty, *Psychol. Rev.*, 43, 1936, 341-365; The interrelation of conditions of difficulty: I. The effect of changes in number at various spatial separations on simultaneous letter span, *J. Gen Psychol.*, 16, 1937, 83-102; and II. Number, spatial separation and illumination as conditions of simultaneous letter span, *ibid.*, 103-130; Two quantitative laws relating to goodness of performance, *J. Psychology.*, 4, 1937, 139-159; and The effect of pattern upon simultaneous letter-span, this JOURNAL, 51, 1938, 83-97.

## I. Effect of Exposure-Time and Number of Letters on Simultaneous Letter Span

The purpose of this section of the study was primarily to determine the effect of a change from 2 to 4 in the number of simultaneously presented letters at various widely different levels of difficulty, the latter being brought about by wide variation in exposure time.[2]

*Method and procedure.* Within certain undetermined limits the time of exposure, as has long been known, exerts only a slight effect upon the percentage of letters correctly named. Obviously, however, if the time is zero, none of the letters could be named correctly, except by chance. It follows that by using some very short times, the difficulty of naming the letters may be made very great. Since a drop-screen tachistoscope was used, the exposure-time depended upon the width of the opening cut out of the cardboard drop-screen. It was not the purpose of the experiment to work out with precision the relation between exposure-time and score, but simply to set up four widely different levels of difficulty, including some of very great difficulty. Consequently, after the longest of these times had been measured by the use of a Hipp's chronoscope and found to be approximately 0.1 sec., the remaining three were estimated simply on the basis of the width of the exposure area. These three remaining exposure-times as thus estimated were all very short; namely, 0.02, 0.015, and 0.01 sec.

Scores from 8 *Ss* were obtained with each exposure-time in the case of both 2-letter and 4-letter combinations. It was desired to obtain an index of the change in difficulty produced by changing the number of letters simultaneously presented from 2 to 4 with the condition consisting of spatial location kept constant. Since no single set of 2 letters can occupy all the spatial positions of a set of 4 letters, the percentage correct for each set of 4-letters has been compared, not with the percentage right for any one set of 2 letters, but with the average percentage right for that pair of 2 letters which would duplicate the set of 4 letters. Since all letters were exposed in a horizontal pattern, with the letters evenly spaced and located symmetrically on both sides of the fixation-point, it follows that if the outermost letters of a 4-letter set consisted of 2 letters 12 cm. apart, the two innermost letters of the set would lie 4 cm. apart. The percentage score with such a 4-letter exposure would be compared with the average percentage correct for two 2-letter exposures, in one of which the 2 letters were separated by 4 cm. and in the other by 12 cm. The letters in those two 2-letter sets, taken together, would duplicate the positions of the letters in the 4-letter set. The difference between the percentage-correct in the case of the set of 4 letters and the average percentage-correct in the case of the 2 sets of 2 letters is considered to be due to the difference in difficulty between 4 letters and 2 letters with the effect of spatial position eliminated.

In one half of the 4-letter sets, the outermost letters were separated by 12 cm. and in the other half, by 10 cm. Since the 4-letters combinations were evenly spaced, the separation of the two innermost letters was 4 cm. in the first case and 3 1/3 cm. in the second. In order to make comparisons of the sort described above, between the scores for 4 letters and those for 2 letters, results were obtained with 2-letter

[2] This study was made with the assistance of Mr. Hubert Brogden.

series with the following 4 separations between the letters: 3 1/3; 4; 10; and 12 cm. Thus, altogether there were 6 types of exposure-slips. The distance of the exposed letters from the bridge of *S*'s nose was 70 cm.; so that the angular separation may be calculated from the fact that the angle subtended by any two symmetrically placed letters is twice the angle of which the tangent is ½ the given separation divided by 70. For example, the angle subtended by 2 letters separated 12 cm. was 9° 48′.

The exposure-slips were arranged in sets of 10, with all the slips of one set containing a given number of letters—either 2 or 4—at a constant spatial separation. All the exposure-slips of one set of 10 were presented before passing on to the next set. *S* was informed in advance of the number of letters and the separation characterizing each set. A 2-letter set contained each of 20 letters. The 6 letters not used were those which were judged by the experimenter from preceding study to be more difficult than the others. Combinations of letters making words were rigidly avoided. A 4-letter set contained the same letters, differently arranged, as the two 2-letter sets with which it was compared. The various sets were presented in a counter-balanced order following the A B B A principle, in which each sort of set occurred 4 times, so that there were 40 exposures of the slips of each of the 6 types at each of the 4 exposure-times, or a total of 960 exposures per *S*. Twelve sets, or 120 slips, were presented at each 50 min. sitting, so that the experiment required 8 hr. of each *S*, in addition to 2 hr. of fore-practice. The experiment was conducted in an artificially illuminated dark-room with an illumination of 13.0 foot-candles at the surface of the exposure-slips. The *S*s named the letters in order from left to right. The instructions, apparatus and other details were the same as in previous experiments.[8]

*Results.* Table I shows the average percentages of letters named correctly, as proportions, for every exposure-time and for each number of simultaneously exposed letters. In addition, it shows the average proportion correct for those two 2-letter separations which taken together would duplicate one of the 4-letter sets.

In spite of a considerable lack of internal consistency in the data, the general outcome is clear and unmistakable. Apart from irregularities, the trend is the same for the 10 cm. and 12 cm. separations, as in the case of the mean for those two separations shown by the bottom line of the table. The conclusion is clearly established that the effect of a fixed change, from 4 to 2, in the condition consisting of number of letters simultaneously presented, has a much greater effect upon the proportion correct when the exposure-time is long than when it is short.

The proportion correct is an index of difficulty, in the sense that the larger the proportion correct, the less the difficulty. A change in the proportion correct, however, is not a safe index of change in difficulty, when the change occurs in proportions of different magnitudes. In order to make the changes in proportions correct more comparable as measures of change

[8] See *op. cit.*, *J. Gen. Psychol.*, 16, 1937, 85-88.

in difficulty, each proportion correct may be expressed as an $x$-value; namely, the distance on the base-line of a normal distribution surface, measured from the mean, which marks off a proportion of the total distribution area equal to the observed proportion correct. The extent to which such $x$-values constitute measures of absolute difficulty is admittedly somewhat conjectural, as indeed is the validity of any measure of absolute difficulty. Among the objections to the use of $x$-values of proportions correct as absolute measures of difficulty is the fact that the different letters are not

TABLE I

PERCENTAGES CORRECT AND THE DIFFERENCE BETWEEN THE 4-LETTERS AND CORRESPONDING PAIRS OF 2-LETTER COMBINATIONS

The column headings, 2 and 4, stand for 2 letters and 4 letters, respectively. Each percentage in each cell is based on 8×80, or 640 letters in the case of 2-letter series and on 8×160, or 1,280 letters in the case of 4 letter series.

| Separation (in cm.) | Exposure-time (in sec.) | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0.1 | | | 0.02 | | | 0.015 | | | 0.01 | | |
| | 2 | 4 | diff. | 2 | 4 | diff. | 2 | 4 | diff. | 2 | 4 | diff. |
| 3⅓ | .894 | | | .784 | | | .438 | | | .225 | | |
| 10 | .566 | | | .306 | | | .113 | | | .097 | | |
| 3⅓ and 10 | .730 | .631 | .099 | .545 | .445 | .100 | .276 | .220 | .056 | .161 | .150 | .011 |
| 4 | .888 | | | .716 | | | .306 | | | .172 | | |
| 12 | .519 | | | .269 | | | .084 | | | .059 | | |
| 4 and 12 | .704 | .580 | .124 | .492 | .414 | .078 | .195 | .300 | −.005 | .116 | .111 | .005 |
| Mean | .717 | .606 | .111 | .518 | .430 | .088 | .236 | .210 | .026 | .138 | .130 | .008 |

all equally difficult. Nevertheless, such scores are certainly superior as indices of difficulty to raw proportion scores, and they will here be used, tentatively, as approximate measures of difficulty, or, what amounts to the same thing in an inverse sense, measures of goodness of performance.[4] The change in the proportions correct due to change in number of letters is indicated, in terms of the $x$-values of the proportions, by Table II. Since it is desired to have the $x$-values refer to goodness of performance, those for scores better than 50% are labelled plus, and those below 50%, minus. These signs are the reverse of those which would be employed if it were desired to have the values represent difficulty. Before transposing the proportions into $x$-values, they were corrected for chance successes by the

[4] See Woodrow, The measurement of difficulty, *Psychol. Rev.*, 43, 1936, 341-365, for a discussion of the assumptions involved in the use of these $x$-values as difficulty scores and for data showing close agreement between these $x$-values and measures of absolute difficulty obtained by Thurstone's method of absolute scaling, applied to data from 169 *Ss* under five widely different constellations of conditions.

formula[5] ${}_{c}p = [n/(n-1)]\,p - [1/(n-1)]$ in which $n$ was taken as 20, *i.e.* the number of different letters used.

From Table II, it is obvious that the effect exerted upon goodness of performance by the fixed change from 2-ness to 4-ness decreased with decrease in the level of goodness of performance at which the fixed change was introduced. Or, in terms of difficulty, as the difficulty was increased (by shortening the exposure-time) it made progressively less difference whether the letters were exposed two at a time or in sets of four. As the exposure-time was shortened, the goodness of performance in the case of 2 letters decreased from +0.530 to —1.324; but at the same time the effect of the change from 2 to 4 letters decreased from 0.314 to 0.053, as represented by the successive values 0.314, 0.235, 0.103, and 0.053.

TABLE II

PERCENTAGES CORRECT TRANSPOSED INTO *X*-VALUES

| Separation (in cm.) | Exposure-time (in sec.) 0.1 | | | 0.02 | | | 0.015 | | | 0.01 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 2 | 4 | diff. | 2 | 4 | diff. | 2 | 4 | diff. | 2 | 4 | diff. |
| 3½ and 10 | +.569 | +.283 | .286 | +.053 | −.213 | .266 | − .713 | − .919 | .206 | −1.190 | −1.253 | .063 |
| 4 and 12 | +.491 | +.146 | .345 | −.087 | −.297 | .210 | −1.025 | −1.003 | .022 | −1.479 | −1.519 | .040 |
| Mean | +.530 | +.216 | .314 | −.018 | −.253 | .235 | −.857 | −.960 | .103 | −1.324 | −1.377 | .305 |

There appears to be no reason for believing that the relationship between level of difficulty and the effect of a fixed change in the conditions of difficulty is dependent either upon the particular conditions which are varied to bring about the different levels of difficulty or upon the particular condition chosen as the one in which the fixed change is introduced at these various levels. Nor is there any reason to suppose that the relationship here discovered is limited to the task of naming letters tachistoscopically exposed. It seems safe to conclude, therefore, that in general the effect of any fixed change in the determining conditions of goodness of performance depends upon the status of the total constellation of conditions, and, more precisely, that the effect decreases as the total constellation of conditions becomes very unfavorable.

## II. A General Law Relating Goodness of Performance to Favorableness of Conditions

The above data establish the fact that the magnitude of the effect of a change in conditions varies with the status of the remaining conditions.

[5] See J. P. Guilford, The determination of item difficulty when chance success is a factor, *Psychometrika*, 1, 1936, 261.

This result is in harmony with a very interesting phenomenon revealed by introducing a fixed change in illumination at various levels of difficulty produced by variation in the number of letters simultaneously exposed from 2 to 6. In this case, the greater the difficulty with normal illumination, *i.e.* the larger the number of letters, the less the effect of a large constant decrease in illumination. It was also found with 6 letters, and possibly with 4 letters (but not with 2 letters), that as difficulty was increased by increasing the spatial separation of the letters, the effect of a fixed change in illumination introduced at the various levels decreased with increase in the separation.[6] Whether the effect of the fixed decrease in illumination varied with the spatial separation of the letters seemed to depend primarily upon the levels of difficulty involved. As long as the difficulty remained low and performance was very good, as with 2 letter exposures, large increases in difficulty produced by increases in spatial separation caused no clearly obvious change in the effect of the fixed decrease in illumination, but as the level of difficulty became pronounced, as with 6-letter exposures, the effect of a fixed change in illumination decreased progressively with further increase in the level of difficulty due to increase in the spatial separation of letters.

On the other hand, it was found in two separate studies that the effect of a fixed change from 2 to 4 letters remained approximately constant over a considerable range of levels of difficulty brought about by variation in spatial separation. Likewise, as noted above, no reliable change in the effect of a fixed change in illumination was produced in the case of 2 letters by wide variation in the spatial separation of the letters. In short, in some instances the effect of a fixed change in conditions introduced at various levels of difficulty remained practically constant over a wide range of levels, while in other instances it decreased with increase in the level of difficulty. An important clue to the reconciliation of these apparently inconsistent results lies in the fact that constancy in the effect of a fixed change in conditions was observed only at relatively low difficulty levels and with relatively small fixed changes. The effect of a fixed change remains approximately constant with variation in the level of difficulty when the latter is low but becomes progressively smaller as the conditions become very difficult. All that is necessary to bring the obtained results into harmony with each other is to formulate the law which governs the relationship between the effect of a fixed change in one condition and the degree of difficulty presented by the total of the conditions at which the change is

[6] Cf. Woodrow, *op cit., J. Gen Psychol.*, 16, 1937, 103-130.

introduced. This relationship, as should be clear from the above, is conceived to be merely a corollary of the relation between goodness of performance and equal changes in favorableness of any one or more of its conditions; and it is a law governing this latter relationship which will be here formulated.

A law which expresses at least the general characteristics of this relationship is represented by the hyperbola of Fig. 1. Any one of several curves might be just as satisfactory, so that the curve here chosen is not to be regarded as necessarily the 'true' curve. It is probably premature to attempt to decide upon the precise formula governing the relations here envisaged. More important is a proper understanding of the two variables which are related, especially as one of them, the $x$-variable, constitutes a dimension not hitherto used in psychological laws.

The $y$-variable is simply performance-score scaled in equal units. Such units are here assumed to be afforded by the $x$-values of the observed percentage scores. All absolute scaling techniques now in use involve the assumption of a normal distribution and therefore yield scores which theoretically could vary from $+\infty$ to $-\infty$. Any curve expressing a relation between such scores and their determining conditions should, then, be a curve which theoretically extends to these infinite limits, even though under actual conditions such scores will never be obtained. This requirement is met by the parabola here used. In a previous paper, the hypothesis that goodness of performance varied logarithmically with unfavorableness of conditions was examined. This hypothesis had to be rejected on account of the fact mentioned above that over a rather wide range in favorableness of conditions, a fixed change in one condition (if not too large) exerted practically a constant effect. It was, therefore, argued that the fundamental relationship was linear and an attempt was made to account for the deviation from linearity, observed at higher degrees of unfavorableness of conditions, by means of several subsidiary hypotheses. Further study has not strengthened any of these hypotheses and the data presented above on exposure-time, showing very pronounced deviations from constancy in the effect of the fixed change from 2 to 4 letters, suggest that one fundamental law will best account for all the obtained results. This law, however, must be one which indicates an approximation to a linear relationship over a wide range of degrees of unfavorableness of conditions at low and moderate levels of difficulty. This consideration appears to be met in adequate fashion by the hyperbola of Fig. 1.

The $x$-axis, in general, represents the number of units of change in favorableness of conditions. In order to make clear the nature of this scale, it will be helpful first to explain a principle of equivalence of changes in conditions; by so doing it will be possible to give a precise, operational definition of this axis. Let the total of the conditions determining goodness of score be represented by $a, b, c \ldots n$, and suppose that these conditions are given such a status that the total constellation of conditions possesses degree of favorableness $F_1$, which determines a scaled performance score $y_1$ (the linear correlation between $F$ and $y$ being $+1$). Now imagine condition $a$, as it exists in $F_1$, to be varied some definite amount, that is, to be changed from one particular value $a_p$ to another particular value $a_q$. Such a change

has here been termed a fixed change. It is not only constant in amount, but a change from one specified value to another. The change in $a$ from $a_p$ to $a_q$ obviously will change $F_1$ to $F_2$ and $y_1$ to $y_2$; but leaving $a$ unchanged, $y$ could be made to change from $y_1$ to $y_2$ by the proper change in any of the other conditions, $b$, $c$ . . . $n$. In other words, it is possible to change from $F_1$ to $F_2$ (therefore from $y_1$ to $y_2$) by some fixed change in any one of the $n$ conditions. A change in any other condition, or set of conditions, which caused $y$ to change from $y_1$ to $y_2$ would be *equivalent* to the change $a_p$—$a_q$. Obviously it would be possible to determine a number of equivalent changes. If, when this had been done, these changes were made *one at a time in succession*, such changes would constitute equal, successive units of the $x$-axis.

Even though only two conditions could be experimentally controlled, it would be possible to lay off a number of units on the base-line $x$. One would begin by determining the equivalent changes $a_p$—$a_q$ and $b_p$—$b_q$. Making the change $a_p$—$a_q$, one would arrive at $x_2$ corresponding to $y_2$. One could then determine the change in $a$ which when applied at $x_2$ would produce the same change as $b_p$—$b_q$ introduced at that same point. In this way, the change $a_q$—$a_r$ which was equivalent to $b_p$—$b_q$ and therefore also equivalent to $a_p$—$a_q$ could be determined. An analogous procedure could theoretically be continued indefinitely, and thus the entire scale laid off in equal units. It is not to be assumed a priori that the amount of change in $a$ represented by the change $a_p$—$a_q$ is necessarily the same as that represented by the change $a_q$—$a_r$. It may be so in some cases, but quite certainly is not so in others.

It is obvious from the above description of the $x$-axis that its units consist of equal changes in the favorableness of conditions. Now the $y$-scores themselves are directly proportional to the favorableness of the total constellation of conditions (and inversely so to what may be termed difficulty); but in determining $y$, various changes such as are indicated on $x$ are made simultaneously—telescoped together—with the result that the total favorableness of the complete constellation of conditions is not proportional to the sum of the separate changes by means of which, analytically considered, the total may be considered as having been built up. Specifically, if one of the two equivalent changes $a_p - a_q$ and $b_p - b_q$ be superimposed upon the other, the change in the favorableness of the total constellation of conditions, and therefore the change in $y$, may not be, and according to the law here proposed, will never be, exactly twice that produced by either of the two equivalent changes taken alone.

In the present work, distances have not been laid off on the $x$-axis in the manner described above. What has been done is to lay off a fixed step on $x$ at various unknown distances apart. It follows that the available test of goodness of fit of any curve expressing the relation between scaled score and the number of units of change in favorableness of conditions is a limited one. It consists solely in ascertaining whether the observed differences in scaled score produced by the fixed change in conditions are at all levels equal to the rise in the curve over constant stretches laid off at the

various indicated positions on $x$. The curve shown in Fig. 1 manifests changes in $y$ for constant $x$-distances, laid off at various regions on the $x$-scale, which roughly approximate the observed changes in $y$, as is made evident by Table III. The curve is the hyperbola obtained from the equation:

$$A x^2 + B x y + C y^2 + D x + E y + F = 0$$

when the constants are given values as follows: A, +6,656; B, —124,090; C, +37,406; D, —202,504; E, +43,158, and F, —64,500. These values were chosen simply as the result of a trial and error procedure and do not give a 'best fit' in the sense of least squares. Both $x$ and $y$ are measured in arbitrary units from an arbitrary zero, though all the $y$ values are the values termed $x$-values in the preceding section, so that the zero point in $y$ means a score of 50% correct. The fixed change in conditions represented by the change from 2 to 4 letters constitutes a constant step estimated at 0.0968 units of $x$.

Table III shows the correspondence between the observed values of $y$ and those calculated by formula. By $y_1$ and $y_2$ in Table III are meant the scores before and after the change in number of letters from 4 to 2, $y_1$ being the score with 4 letters, and the corresponding $y_2$ the score made (under otherwise the same conditions) with 2 letters. Since the change from 4 to 2 letters constitutes a fixed increase in favorableness of conditions, no matter at what level it is introduced, it should be represented by a constant distance on the $x$-axis. Using as an estimate of this distance 0.0968 units, this step has been laid off on the $x$-axis beginning at the values of $x_1$ calculated by formula from the corresponding values of $y_1$; and the ensuing values of $x_2$ (the values obtained by adding 0.0968 to $x_1$) have then been translated by the formula into estimated values of $y_2$. The discrepancies between the observed and estimated values of $y_2$ are small. As indicated in the table by the last column to the right, the largest discrepancy is no greater than could be accounted for by an error of 1.9% in the original proportion correct from which the $y_2$ value was calculated. In other words, once the effect of a fixed change in conditions, introduced at any one level of goodness of performance, has been experimentally determined, the curve permits an estimate of the effect which will be produced by the same change at any other level.

Values of $y_1$ beyond +0.216 are placed in Table III in order to permit a prediction by formula of the effect on $y$ of the change from 4 to 2 letters at high levels of goodness of performance. Since study of the effect of change from 4 to 2 letters at these high levels of favorableness was

conducted with other groups of *Ss* than the one used in the above described study of variation in exposure-time, and since, further, the magnitude of the effect of this change will vary from one small group of *Ss* to another, no actual data concerning the effect of the change from 4 to 2

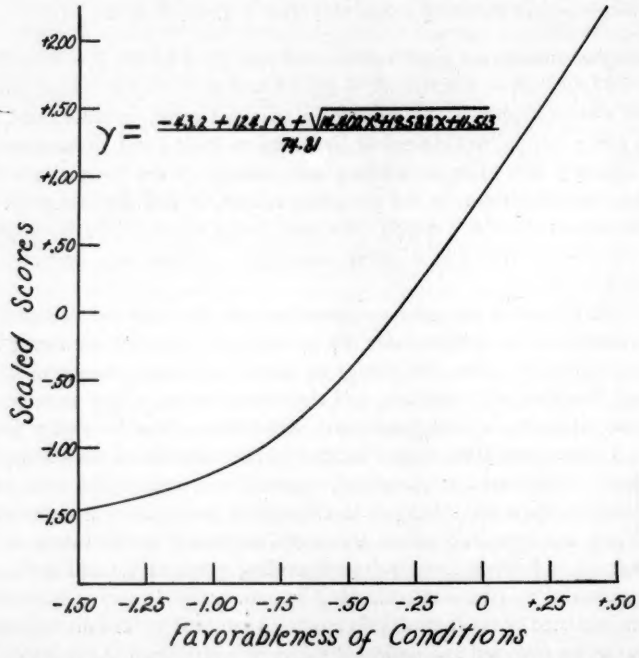


FIG. 1. CURVE SHOWING THE RELATION BETWEEN THE SCALED GOODNESS OF PERFORMANCE ($y$) AND NUMBER OF UNITS OF CHANGE IN THE FAVORABLENESS OF CONDITIONS ($x$).

The effect of a fixed change in any one condition (or an equivalent change) is equal to the rise in the curve subtended by a constant distance on the base-line or $x$-axis. The effect of the change from 4 to 2 letters, when this change is made at various levels of difficulty, corresponds to the increases in height shown by those portions of the curve subtended by equal stretches (0.0968 units in length) located at different positions on the base-line.

letters at these levels is incorporated in Table III. In the previous studies, however, it was found that at these levels of $y_1$ (*i.e.* from near zero to +1.8) the effect of this change from 4 to 2 letters was approximately constant. The predicted effects (*i.e.* $y_1 - y_2$ differences) show a progressive

increase instead of the constancy observed in the previous studies, but the increase is very slight (from 0.262 to 0.296) and could well have been obliterated by experimental errors. For example, to make the largest value in this range (0.296) equal to the smallest (0.262), would require a change in the percentage-correct score corresponding to the highest $y_2$ (+2.096) of only 2/10 of 1%, *i.e.* from 98.2 to 98.0.

Incidentally, it may be observed that no evidence has been found of a negative acceleration in the rate of rise of the curve at extremely high levels of favorableness of conditions. Instead, it seems likely that at extremely high levels of favorableness the curve keeps on rising almost in proportion to the increase in the $x$-axis values.

TABLE III

COMPARISON OF OBSERVED CHANGES IN SCALED SCORES ($y$) WITH CHANGES CALCULATED ON ASSUMPTION THAT THE CHANGE FROM 4 TO 2 LETTERS EQUALS 0.0968 $x$

| $y_1$ | $y_2$ | $y_2$ (If $\Delta x$=0.0968) | $y_2-y_1$ | $y_2-y_1$ | $y_2-y_2$ (In terms of $p$) |
|---|---|---|---|---|---|
| −1.377 | −1.324 | −1.327 | .053 | .050 | −.000 |
| − .960 | − .857 | − .820 | .103 | .140 | +.010 |
| − .253 | − .018 | − .020 | .235 | .233 | −.001 |
| + .216 | + .530 | + .478 | .314 | .262 | −.019 |
| + .6 | | + .875 | | .275 | |
| +1.2 | | +1.489 | | .289 | |
| +1.8 | | +2.096 | | .296 | |

As is obvious from Fig. 1, the effect of a large fixed change in favorableness of conditions will show a larger decrease with decrease in favorableness of conditions than will a small fixed change. For example, as $y$ decreases from +2.362 to —0.431 (and $x$ from +0.5 to —0.5), the effect on $y$ of an unfavorable change on the $x$-axis as small as 0.0968 $x$-units decreases only 0.102 $y$-units; whereas the effect on $y$ of a change on the $x$-axis as large as 0.5 $x$-units, decreases as much as 0.764 $y$-units. These relations may be made quite obvious graphically by drawing curves 0.0968 units and 0.5 units, respectively, to the right of the curve shown in Fig. 1. The vertical distances between the curves show the effect at various levels of difficulty of a fixed change equal to the horizontal distance between the curves. The relations thus revealed agree with those experimentally obtained. Thus, while the change in the effect on $y$ of the fixed change from 2 to 4 letters was not detected as long as $y$ was above zero, the effect of the larger change from 2 to 6 letters became considerably smaller at the lower levels.[7]

In summary, then, it may be said that all the data so far obtained on

[7] *Op. cit., J. Gen Psychol.*, 16, 1937, 110.

the relation between the goodness of performance in the case of simultaneous letter span and variations in the determining conditions of this performance may be accounted for by one fairly simple law, expressing in the form of a hyperbola the relation between two variables. This law is one relating goodness of performance in equal, or scaled, units (the $y$ variable) with units of change in favorableness of conditions (the $x$ variable). Whatever be the proper, precise, quantitative relation, the curve expressing this law should have the following characteristics: it should permit $y$ values extending from $+\infty$ to $-\infty$; it should approximate a straight line at high values of $x$; and it should show a negatively accelerated rate of decrease in $y$ with decrease in $x$. It is proposed, then, that there exists a law which prevails in the case of all measurable task-performances between units of goodness of performance and units of favorableness of the determining conditions, to the effect that, as the conditions are made unfavorable, goodness of performance drops at first rapidly and almost proportionally to the number of units of decrease in the favorableness of conditions; and that as the number of units of decrease in favorableness of conditions becomes relatively large, the drop in goodness of performance occurs at a progressively slower rate.

# CONDITIONING AND PROBLEM-SOLVING BEHAVIOR

By EDWARD GIRDEN, Brooklyn College

The present investigation was initiated as a result of observations made during the establishment of the auditory localization habit in dogs in a relatively free conditioning situation.[1] Briefly, the animal was trained to flex its forepaw upon the presentation of a conditioned stimulus (CS)[2] from its right side, but to inhibit this response whenever the sound was presented from its left side.

The essentials of the technique have been described elsewhere,[3] but since certain modifications have been made, the method merits restatement here. Restraint of animal was limited to immobilization of forepaws in such fashion that flexion was possible only in the vertical direction.[4] The forepaws rested upon metal grids, and the indifferent electrode, covered with gauze soaked in saline, was attached to the left or right forepaw as the case required. The buzzer (CS) was placed just in front of the right forepaw, above the shock-grid. The apparatus could be controlled automatically in which case the CS was sounded for 2 sec. and followed by a momentary presentation of the unconditioned stimulus (US)—an electrical shock. Appropriate switches made possible the manual presentation of any combination of these stimuli as desired.

The intensity of the shock was varied, as required, so as to limit the response

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[1] A preliminary report of these findings was made at the meeting of the Eastern Branch of the American Psychological Association in New York, April 1, 1938.

[2] The following abbreviations are used in this study: US = Unconditioned, original, stimulus (shock); UR = unconditioned, original, response to US; CS = conditioned, substitute, stimulus, such as pure tone, or buzzer; CR = conditioned response to CS; RS = (conditioned) sound stimulus presented from the dog's right side; LS = (conditioned) sound stimulus presented from the dog's left side; L-R habit = auditory localization discrimination whereby the dog responds (CR) to RS, but does not respond to LS.

The 'shock-noise' was a characteristic sound, unlike those produced by the CS buzzers, presented simultaneously with application of shock during some of the conditioning procedures. In the present series of experiments, it was not used except in the advanced stages of developing the L-R habit. This stimulus actually serves as CS for paw extension: *i.e.* the animal keeps its paw flexed until the sound is given. Overactivity is often successfully eliminated by presenting this stimulus alone.

[3] E. A. Culler, G. Finch, E. Girden and W. Brogden, Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

[4] In the original experimental situation, body supports, leg-straps and wooden neck-pieces (head-stock) were used, and shall be referred to in the present paper, as the 'restraint' technique. The present method shall be designated as the 'free situation.' The data procured in the two situations are also differentiated by the year in which they were procured (cf. footnote 7). When the former technique was used in the present experiment, it is so indicated by the description, "restraint."

to flexion of forepaw alone. During training with shock-alone, the US was applied only when the animal was quiet and, if possible, when oriented to the shock-grid. Successive presentations were made at irregular intervals so as to avoid temporal conditioning. During actual conditioning, the sound of the CS would often be prolonged beyond the 2-sec. interval until the dog oriented to it, before the US was applied. If the dog flexed its paw to the CS, the terminal shock was avoided. A test-period ordinarily lasted 10–20 min., during which time 25–30 presentations of the stimuli were made.

## RESULTS

*(1) Behavior produced by the unconditioned stimulus (US).* It is possible to present here only a more general description of the behavior (UR) that develops from repeated stimulation with shock (US) alone. While a quantitative description would appear to satisfy the need for objectivity of report, such a statistical enumeration of the responses made would be far from a true portrayal of the animal's total behavior. The dog appears to go through a series of, far from senseless, attempts to solve the 'problem.'[5] With extreme restraint, such behavior is almost completely masked tending to appear only as senseless generalized struggle behavior.

Some of the characteristic patterns employed are noted from the protocols: (1) attempts at pulling away from the front of the apparatus (which, due to the restraint of the forepaws, is impossible); (2) turning away of head from the front part of the body; (3) tendency to lean upon forepaw being shocked (extensor thrust?) with concomitant relaxation of opposite forepaw; (4) tension of respective forepaws varied upon successive trials, head being held in various positions, such as resting on floor and parts of the apparatus, or looking at the shock-grids; (5) various combinations of these patterns while the animal is standing, squatting or reclining completely.

In all the animals tested in this situation, a characteristic 'set' eventually appeared. The animal's body assumed a *U* shape, with head pointed and leaning toward the shock-grid through which the US was applied (thus the head was turned to the right when the US was applied to the right forepaw); the rear legs were similarly placed when the animal was erect, and the center of the body leaned and bent towards the opposite side of the platform. The animal rested lightly upon the forepaw to which the US was applied, with most weight placed on opposite paw. Variations in behavior of course occur among the different animals, but there was in every case a definite spatial organization of the body with respect to shock-grid. The response of the forepaw to the US was at first completely reflexive. With continued tests, a voluntary component appeared; after paw

[5] See I. Krechevsky, A study of the continuity of the problem solving process, *Psychol. Rev.*, 45, 1938, 107-133.

was reflexively withdrawn from shock, it was withheld for a varying interval of time before being replaced. This latter response was acquired after the organized body set was established. The degree of stability of this final pattern was a function of the number of trials and could be disrupted by the application of shock to the opposite forepaw. If this latter procedure was continued the opposed body set was developed.

*(2) The conditioned response (CR) after training with the unconditioned stimulus (US).* Let us now consider the protocols of the animals conditioned in the free situation after varying amounts of training with shock-alone. It will be seen that the forepaw CR developed very rapidly, approaching 100% before the conclusion of the first test-period (25–50 trials). The type of CR procured depended upon the degree of preliminary training with the US. If the UR was still at the reflexive stage, then the CR assumed the same form. If, however, sufficient preliminary training with the US had been first given, then the voluntary CR appeared in short order.

*Dog. No. 10.* Mar. 5-23, 1938 (10 test-periods): after a total of 250 trials of US to right forepaw, the body set to right shock-grid was established, and paw flexion was largely, though not as yet completely, voluntary. Interspersed, during these tests, were 35 presentations of buzzer alone (never followed by US) near right forepaw, producing characteristic head orientations without flexion of paw.

Conditioning was initiated Mar. 23 with the following results: 0/5, 2/5, 2/5, 5/5, 4/5 CR in the first trials and 24/25 CR on the next trials.[6]

*Dog No. 7.* Feb. 24–25, 1938 (2 test-periods): total of 40 trials of US alone to left forepaw, followed by 20 trials of US alone to right forepaw on Mar. 2. March 3–9 (3 test-periods): 30 more trials of US to right forepaw. During these latter periods, conditioning to buzzer was initiated and CR developed as follows: 0/5, 2/5, 4/5, 2/5, 5/5 CR on the first day, and 20/20 CR on the second test-day. Response to shock was reflexive, without a voluntary component. As far as could be observed, the CR possessed the same characteristics as the UR. With continued training (after a total of about 200 trials) the left forepaw CR developed into a voluntary response with the appearance of the characteristic body set.

*Dog No. 9.* Feb. 26-Mar. 19, 1938 (8 test-periods): US alone applied to right forepaw. Body set and voluntary paw flexion became well established. At the conclusion of the 8th test, the US was now applied 20 times to the left forepaw. This immediately began to disrupt the body set. Conditioning was then initiated (US applied to left forepaw) with the following results: 0/5, 1/5, 1/5, 3/5, 3/5 CR on the first 25 trials and 24/25 CR on the second 25 trials of the first test-period. On the next four days the score was a total of 104/105 CR. These consisted of combinations of left and/or right forepaw flexions, alternately, in series, or in a variety of combinations. This continued for the better part of the next two weeks, before the right forepaw response was completely eliminated. As the original pattern disappeared (of which the right forepaw response was a part) the left forepaw CR developed voluntary characteristics, paralleling the development of the new body set to the left grid.

[6] If the paw was flexed once or oftener during the sounding of the CS, it was considered as a CR, whether or not the terminal shock was avoided. Numerators of fractions indicate the number of CR, the denominators the total number of trials.

*(3) Perceptual organizations during the process of conditioning.* The acquisition of the L-R habit has been studied in two situations: (1) when the CR had been developed from the first with the CS sounded from a position near the right forepaw; and (2) when the L-R habit was tested only after the CR had been first established with the CS presented from a position in front of the test-stock. While the findings raise more problems than they answer, they do indicate that the behavior may become organized to particular attributes of the stimulus-field after training with only the positive stimulus (RS).

*(a) Development of L-R habit after conditioned response (CR) to right side (RS) was established.* When the conditioned response (CR) to right side (RS) was established, it was followed by tests with the left side (LS). The following protocols are the scores of the tests with the buzzer on the LS in animals previously conditioned to RS. Only a few of these animals had been given preliminary training with US alone.

*Dog No. 5.* (Right forepaw CR to RS already established.) Nov. 17–29, 1937 (11 test-periods): no paw flexion responses were produced with LS buzzer (100 trials). Buzzers then were switched, but the discrimination persisted undisturbed: 100% CR to new RS buzzer, and 0% (no) CR to new LS buzzer.

*Dog. No. 4.* (Right forepaw CR to RS already established.) Nov. 19–21, 1937 (3 test-periods): no paw flexion responses were produced with LS buzzer (50 trials). On the next seven test-days, the total score was 12/82 CR; 7 of these responses occurring on one test after the US has been accidentally applied on one trial. On the whole, there was a definite discrimination between the RS and LS stimuli, but when the buzzers were interchanged the dog now responded to the new LS, indicating that the discrimination in the first instance was not in terms of spatial locus, but the tonal qualities of the sound-sources.

*Dog No. 6.* (Left forepaw CR to RS already established.) Nov. 26-Dec. 2, 1937 (3 test-periods): scores with LS, 1/1, 1/5, and 1/15. Thereafter the dog was entirely satisfactory, responding only twice out of the next 200 trials during the 19 tests made. These tests included trials with the buzzers switched, substitution of new buzzers, and the introduction of pure tones. Although these data might not be considered as evidence for the immediate appearance of the L-R habit (since some responses were evoked by LS), this dog was very close to perfection.

*Dog No. 7.* (Right forepaw CR to RS already established.) Since this animal has not been given many preliminary trials with US alone, the body set had not as yet been completely developed when the first trials with LS were made.

Mar. 18-21, 1938 (3 test-periods): scores with LS as follows, 1/5, 0/5, 4/5. While these results were not particularly bad, it was decided to first transfer the animal to the restraint situation, before continuing the tests. In the shift to the new situation, some rather interesting phenomena occurred which are significant in their own right.

From inspection of Table I, it is clear that the CR was not paw flexion to just an auditory stimulus, but specific to limited aspects of the situation-as-a-whole. When the CS was taken from the grid position and placed on level with the dog's right ear (CS at ear), one shock trial was required (Mar. 22) before the dog would respond to it in this new position. In the restraint situation, four days of testing were required before the animal would respond to the buzzer sound. That

the CR was not 'lost' during this time is indicated by the interpolated tests made during the same days with the dog in the free situation. The two situations had obviously different meanings for the animal. Once the dog 'caught on' in the new situation, the transfer was sudden and complete. Whether the basis of this organization was perceptual (spatial attribute) in nature or due to the 'body set' is hard to say at this time. The present data may be used to support either hypothesis, and more unequivocal tests in this situation are required before a definite statement can be made.

When the animal was now established in the restraint situation, tests for the localization habit were again initiated. When the CR had been developed with restraint on Mar. 26 (cf. Table I) LS tests were made with the following results: 2/15. Four days later, regular tests with LS and RS were begun. On the first test the score with LS was 1/20; thereafter, no errors were made at all. In short, during

TABLE I

RESULTS OF DOG NO. 7 IN 1938

This animal possessed a well established CR to CS at grid, in free situation, when these tests were started. Cf. footnote 2 for meaning of the abbreviations, and text for discussion.

| Date | Free situation | | Restraint | |
|---|---|---|---|---|
| | CS at grid | CS at ear | CS at grid | CS at ear |
| Mar. 21 | 14/15 | | | |
| 22 | 6/6 | 0/1, 3/3 | | 0/20 |
| 23 | 5/5 | | | 0/25 |
| 24 | 10/10, 5/5 | | 0/25 | |
| 26 | 5/5 | 6/6 | 5/5 | 0/1, 13/14 |

all these tests, the dog responded but 8/55 trials. That this discrimination was a true localization habit is indicated by perfect scores attained with buzzers switched, and chance order presentation of LS and RS stimuli.

*Dog No. 9.* (Left forepaw CR to RS already established.) Scores with LS were as follows, 4/5, 3/10, 0/5, 0/10, 2/20 CR (the two responses on the last test—2/20—were the first two trials). Thereafter the score was perfect, and again, as with the previous animal, the proof that this discrimination was true auditory localization is indicated by the fact that LS scores remained at zero (no responses) upon switching of sound-sources and chance presentations of stimuli.

*Dog No. 10.* (Right forepaw CR to RS already established.) While the animal was still in the free situation, scores with LS were, 1/5. Several days later, after transfer to restraint had been satisfactorialy effected, tests with LS and RS were made regularly, with the usual controlled presentations of stimuli in chance order. On every trial, no response was made to buzzer sounded on left side. Again, that it was truly a L-R habit was shown by similar results with switching of the sound sources.

*(b) Development of L-R habit after CR to CS placed in front had been established.* In contrast with the previous data are the findings resulting from tests with animals originally conditioned to an auditory stimulus presented from a position some three feet in front of the test stock.[7] After the CR was developed, training

[7] The results discussed in this section were procured at the Physiological Psychology Laboratory, of the University of Illinois, and are referred to by year (1936) to distinguish them from the present data which are designated by 1937 or 1938.

with LS and RS buzzers were started. Even a superficial examination of the data in Table II indicates clearly that we are dealing with a definitely different phenomenon. In *Dog No. C,* we can see evidence for the rapid appearance of a discrimination in terms of the difference in tonal quality of the two buzzers. This was not, however, a true L-R habit, for with switching of the buzzers (indicated in Table II by the asterisk (*) the CR to LS reappeared. The L-R habit developed slowly. As far as the tests went, only one of these three animals developed the L-R habit to a satisfactory level.

TABLE II

DEVELOPMENT OF L-R HABIT AFTER CR TO CS IN FRONT OF ANIMAL WAS ESTABLISHED (1936

CR established first to CS presented from in front of animal, before RS and LS training was initiated. US applied if no response was made to RS, but no reënforcement was applied to LS. The asterisk (*) indicate times at which RS and LS buzzers were switched. Cf. footnote 2 for meaning of the abbreviations, and text for discussion.

| Test No. | Dog No. A | | Dog No. B | | Dog No. C | |
|---|---|---|---|---|---|---|
| | RS | LS | RS | LS | RS | LS |
| 1 | 8/10 | 2/10 | 12/15 | 3/16 | 16/25 | 0/20 |
| 2 | 10/10* | 3/13 | 7/15* | 3/15 | 16/22 | 0/20 |
| 3 | 10/10 | 19/37 | 10/10* | 7/15 | 21/25* | 3/25 |
| 4 | 10/10* | 16/28 | 9/11 | 0/12 | 9/10 | 5/20 |
| 5 | 15/16 | 16/40 | 10/15* | 9/25 | 7/8 | 2/16 |
| 6 | 10/14 | 4/25 | 7/15 | 3/15 | 11/14* | 0/13 |
| 7 | | | 5/15 | 13/30 | 15/20* | 18/52 |
| 8 | | | | | 8/8* | 2/18 |
| 9 | | | | | 21/21* | 2/25 |
| Totals | 63/70 | 60/153 | 60/86 | 38/128 | 124/153 | 32/209 |

Three other dogs trained during the same time (1936) required 300 to 400 trials with RS and 250 to 580 trials with LS (presented in more or less random order during any one test) over a period of from 24 to 26 tests, before the L-R habit was completely developed. These dogs, likewise, had been first conditioned to CS sounded from in front of the test-table. In none of these six animals was there any evidence for the immediate establishment of the L-R habit.

In comparing the results procured in the two situations it is evident that the learning involved a perceptual organization, in terms of either the tonal or spatial qualities of the stimuli presented. By placing the CS in a particular position, the behavior became more readily organized with respect to its locus. The rapidity with which the CR to LS was eliminated was due, not only to the spatial locus of the CS, but also to the introduction of a special technique, which had not been employed in the earlier situation (1936). This method and its significance is discussed below.

*(4) Reorganization of behavior in the development of the L-R habit.* Suppose the dog was found to possess paw-flexion to the first presentation of LS. Instead of terminating the sound at the usual time (2-sec. presentation), it, on the contrary, was continued for some time. The behavior to this continued stimulus was characteristic of the animals observed in this

situation. Paw-flexion might occur as a continued single, or series of rapid flexions of the paw interspersed with head and visual orientations to the left and right sides of the apparatus.

These 'lookings back and forth'[8] were quite characteristic of the dogs in this situation. While no attempt has been made as yet to study this phenomenon in detail, the observations to date indicate that the elimination of paw-flexion to LS proceeded rapidly with its appearance. In some cases, although the orientation appeared, there was 'reflexive' withdrawal of paw (in contrast to the more usual voluntary response), as if the animal had 'caught on' but could not inhibit the activity. This was particularly striking in the case of *Dog No. 3*, who had shown much generalized excitement during the original training because of the use of the restraint-technique.

When the paw had been finally replaced upon the grid with no further observable tendency to paw-flexion, the sound of the buzzer was terminated. This technique was of considerable value in establishing the localization habit in those animals whose discrimination was basically one of tonal difference between the two buzzers. Quantitative data from two dogs are offered below as typical of the results procured in this situation.

*Dog No. 1.* (Right forepaw CR to RS already established.) Nov. 28–Dec. 13, 1937. Training scores with LS as follows, 5/11, 1/5, 0/8, 0/10, 0/10, 0/8, 1/10. The two buzzers were then switched and the scores with the new LS were, 6/30, 1/12, 0/10, 0/10. The localization habit then remained undisturbed with further interchanging of sound sources. When a new pair of tonal stimuli were introduced, the scores with the LS were as follows, 1/22, 2/20, 0/15, 1/12. The L-R habit persisted undisturbed thereafter, regardless of the order of presentation of stimuli or the position of the stimuli. In all these tests, the CR to RS was maintained uniformly close to 100%. This animal obviously first possessed a tonal discrimination, which was rapidly transformed into a true L-R habit.

*Dog No. 3.* (Right forepaw CR to RS already established.) Dec. 3–16, 1937. Scores with LS were, 1/1, 6/10, 24/25, 19/40, 4/20, 2/20, 0/11, 0/8. Buzzers were then switched with the following LS results, 4/25, 0/21. Animal was satisfactory thereafter with further interchanging of sound-sources, introduction of new stimuli, and even during tests with pure tones. The more gradual development of the L-R habit in this animal can be ascribed to the presence of generalized struggle behavior which resulted in part from excessive application of US during early training.

This technique was particularly effective in training the animals to localize pure tones above 1024~ per sec., which seemed to be more difficult because of the poor acoustical properties of the test room. As a casual observation, it seems significant that in no case in which the L-R habit was firmly established (1937-38) was the unconditioned head-orientation to CS (buzzer or pure tone) ever completely eliminated. In some cases it was reduced to an almost imperceptible response. Thus, *Dog No. 3* (from the protocols): LS produced eye orientation to the left, body

[8] E. C. Tolman, The determiners of behavior at a choice point, *Psychol. Rev.*, 45, 1938, 1-41.

remaining motionless. With presentation of RS, eyes turned in that direction, followed by characteristic CR 'flip' of right forepaw.

## Discussion

Culler and his associates have presented much evidence showing that shock, as the US, serves in several capacities.[9] The present experiment, in part, is concerned with one phase of this question. The development of motor conditioning in dogs has been described as a process during which the initial generalized behavior is finally replaced by localized paw-flexion. Such a process has not been observed in the totally decorticate dog. The CR in such an animal remains at the initial unorganized level.[10]

Kellogg has recently reported that localized paw-flexion CR can be procured without initial struggle behavior. He suggests that there are two types of CR: "the conditioned flexion movement essentially the same as the unconditioned flexion movement," and secondly, a "voluntary reaction."[11]

The present data indicate that localized paw-flexion can be produced without *observable* diffuse behavior. This is a function of the degree of restraint and intensity of shock used, as well as possible individual differences between animals.

The individuation of the localized paw response, in the first place, is not due to conditioning, but results from repeated application of shock alone. When conditions are manipulated so as to limit the observable response to flexion of paw alone, it is still safe to conclude that some such process occurs. For, in every case, the dog develops a definite organized body set which eventually includes a voluntary withdrawal of paw upon presentation of the shock stimulus. This organization of behavior, as shown by the present data, is clearly a function of the US alone, and Kellogg's description of two types of CR simply represents two stages in this process. It is this organized behavior which the decorticate animal is incapable of developing.

The use of restraint has obscured the observation of the development of this behavior, with the consequent error of assuming that the preparatory set is a product of the conditioning process. The use of restraint in some animals inhibits completely the development of this behavior and the localized adaptive paw flexion.

[9] E. A. Culler, Recent advances in some concepts of conditioning, *Psychol. Rev.*, 45, 1938, 134-153.

[10] E. Girden, F. A. Mettler, G. Finch and E. A. Culler, Conditioned responses in a decorticate dog to acoustic, thermal and tactile stimulation, *J. Comp. Psychol.*, 21, 1936, 367-385.

[11] W. N. Kellogg, Evidence for both stimulus-substitution and original anticipatory responses in the conditioning of dogs, *J. Exper. Psychol.*, 22, 1938, 190.

Thus Schlossberg failed to procure this behavior in the albino white rats, with or without constant shock-reënforcement,[12] and Liddell, James and Anderson report that "in the rabbit [the CR] never becomes stable."[13] Similar negative results have been procured in the cat. In all these cases, extreme restraint was used. When the restraint is modified, as Bromiley of Johns Hopkins reports,[14] adaptive paw flexion CR is readily developed in the cat. In the light of these findings, as well as the present data showing a more rapid development of the CR under conditions of modified restraint, there is every reason to believe that similar results should be procured with the white rat and the rabbit.

The instability of the paw-flexion CR reported by Liddell, James and Anderson is due also to another factor. According to them "the overt response (paw flexion) may not appear until the third or fourth application of the reënforced signal."[15] That "even one omission of the mildly noxious stimulus when the conditioned stimulus has been presented exerts a clearly perceptible effect upon the nature and magnitude of the reaction to the same stimulus presented a few minutes later."[16] And, "the most important factor in maintaining the conditioned reflex at a constant level is the reënforcement of the conditioned stimulus at every presentation."[17]

In these experiments constant reënforcement was employed; that is, shock was applied on every trial, whether or not the animal flexed its paw to the conditioned stimulus. This attempt to imitate the Pavlovian tradition actually violates these tenets. As Culler has shown a muscle contracted as a result of shock application tends to persist for a period of time in that condition.[18] Application of the CS, the bell, during that interval produces not further contraction but relaxation. In other words, by applying shock when animal *has* responded, one is really attempting to establish an extension CR. Liddell's own observation seems to bear this out. He reports that "if the sheep is placed on its back and the conditioned stimulus is given . . . all four limbs are stiffened."[19] In view of the extreme instability of the paw-flexion CR produced with constant shock-reënforcement, and failure of the CR to appear at the first trial of a particular session, it is likely that these animals are really being reconditioned at every test. From the point of view of 'problem solving' behavior these results could likewise be predicted. Constant shock-reënforcement permits no solution of the situation and hence stable learning cannot be expected to develop.

Proceeding to a consideration of the present data concerning the rate of conditioning, it is evident that the process proceeds very rapidly. With little restraint, the organization of the CS and US in the same visual field, and continued presentation of the CS until the animal orients to it before

[12] H. Schlossberg, The relationship between success and the laws of conditioning, *Psychol. Rev.*, 44, 1937, 379-394.

[13] H. S. Liddell, W. T. James and O. O. Anderson, The comparative physiology of the motor conditioned reflex, *Comp. Psychol. Mono.*, 11, 1934 (no. 51), 26.

[14] Personal communication to the writer. These results of Bromiley's are to appear in the near future.

[15] Liddell, James and Anderson, *op. cit.*, 33.

[16] *Ibid.*, 27.

[17] *Ibid.*, 43.

[18] E. Culler, *op. cit.*, p. 140.

[19] H. S. Liddell, The conditioned reflex, in *Comparative Psychology*, F. A. Moss (Ed.), 1934, 273.

the US is applied, the paw-flexion CR can be developed to approximately 100% within a single test-period. Other data (not as yet published) were procured in relatively the same free situation. The CS was sounded near the animal's right ear, but the presentations were made in the traditional manner: buzzer for 2 sec. followed by momentary shock. In this case the CR does not develop as rapidly. The exact significance of each of these factors—lack of restraint, visual organization of stimuli, and continued presentation of CS—must, however, await the test of more crucial experiments.

The effectiveness of the perceptual organization is shown more clearly in the development of the auditory habit of L-R localization. If the dog is trained with the substitute stimulus placed on its right side from the very first test (RS), the L-R habit may develop without any training with the second, left, stimulus. In other cases, little training is required to extinguish the CR to the stimulus from the left side. Extended presentation of LS, when the CR was evoked by it, was found to be of considerable significance. The change in behavior in this situation is at present difficult to interpret in terms of conditioning. It seems evident that by assuming the situation is *a problem for the animal and not the experimenter,* and by acting accordingly, that one can produce changes in behavior more rapidly than can otherwise be obtained.

## Conclusions

The organization of the anticipatory body set and the voluntary paw-flexion response is shown to be a function not of conditioning but repeated application of shock alone.

The rate of development of the paw flexion CR is a function of the amount of restraint, and the spatial and temporal properties of the stimuli used. Under proper conditions, 100% CR can be approximated within a single test-period (25–50 trials).

By establishing optimal conditions, immediate perceptual organizations may appear which have considerable significance for the later development of the L-R auditory localization habit.

# VISUAL DIFFERENTIAL SENSITIVITY AND RETINAL AREA

By ALFRED H. HOLWAY and LEO M. HURVICH, Harvard University

Results which demonstrate the dependence of differential sensitivity ($=1/\Delta I$) upon the size of the retinal image have been reported by Aubert, Abney, Simon, Lazareff, Heinz and Lippay, Reeves, Cobb and Moss, Lowry, and Steinhardt.[1] Although the experimental conditions differ in details, the data of these workers in general show clearly that, for intensity and exposure-time as parameters, $1/\Delta I$ varies directly with the size of the retinal image.

The results for the discrimination of just noticeable differences in brilliance were usually secured, however, either for a few intensity-levels, for a narrow range of retinal areas, or for both. Steinhardt's work provides the single exception. This experimenter explored a wide range of retinal sizes (16′ to 25°) and intensities (0.001 to 20,000 ml.) for a single *O*. One retinal area was tested at a given sitting for different levels of illumination. The results are thus homogeneous relative to retinal illumination. They are not homogeneous for area. Measurements for a wide range of intensities which are homogeneous for retinal areas are presented and considered in the following report.

*Apparatus and procedure.* The apparatus was designed to permit successive presentation of the stimulus-fields. See Fig. 1 for schematic plan. Two 500-watt Mazda clear projection lamps, $S_1$ and $S_2$, enclosed in galvanized iron housings, served as light sources. The light from each source was diffused by flashed opal plates at $D_1$ and $D_2$, and then deflected 90° at the polished aluminum-surfaces, $R_1$ and $R_2$. The illumination of the test-fields was controlled (1) by moving lamps along the length of the housings and (2) by inserting neutral tint filters at *F*. A fixation-point was placed at *P*.

Successive presentation of the two test-fields was effected in the following manner. The aluminum-surface at $R_1$ was attached to a solenoid of the magnetic cushion type. When the solenoid was unactivated, the light was reflected from the surface

* Accepted for publication November 3, 1937.

[1] H. Aubert, *Physiologie der Netzhaut*, 1865, 82-89; W. Abney and W. Watson, The threshold of vision for different coloured lights, *Philos. Trans. Roy. Soc.*, 216A, 1916, 91, 128; R. Simon, Über die Wahrnehmung von Helligkeitsunterschieden, *Zsch. f. Psychol.*, 21, 1899, 433-442; P. Lazareff, Studien über das Weber-Fechner'sche Gesetz: Einfluss der Grosse des Gesichtsfeldes auf den Schwellenwert des Gesichtsempfindung, *Arch. f. d. ges. Physiol.*, 142, 1911, 235-240; M. Heinz and R. Lippay, Über die Beziehungen zwischen der Untershiedsempfindlichkeit und der Zahl der erregten Sinneselemente, *Arch. f. d. ges. Physiol.*, 218, 1928, 437-447; P. W. Cobb and F. K. Moss, The four variables of the visual threshold, *J. Franklin Inst.*, 205, 1928, 831-847; E. M. Lowry, Some experiments with binocular and monocular vision, *J. Opt. Soc. Amer.*, 18, 1929, 29-40; J. Steinhardt, Intensity discrimination in the human eye: I. The relation of $\Delta I/I$ to intensity, *J. Gen. Physiol.*, 20, 1936, 185-209.

at $R_1$; when it was activated, $R_1$ was abruptly elevated, and the light was reflected from the aluminum-surface at $R_2$. An automatic timing device, operated through a relay, controlled the duration of exposures. The exposure-times of the continuously alternating standard and adjusted test-fields were 2 sec. and 1 sec. respectively.

A large white mat-surface (visual angle = 90° x 60°) at *A* illuminated by four 75-watt daylight Mazda lamps served as background for the test-fields. The brightness of the background was equated to that of the test-field before each

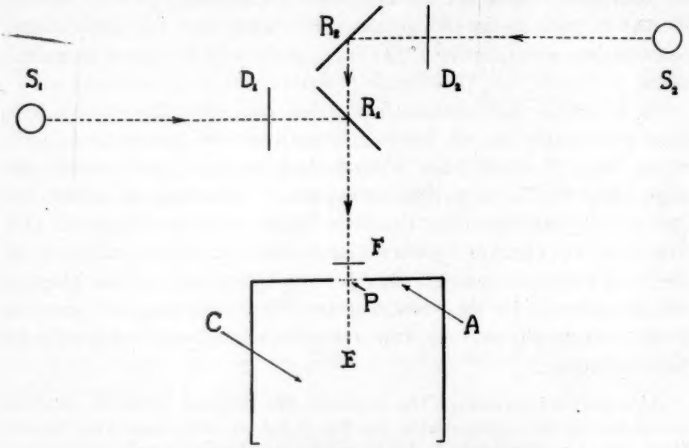


FIG. 1. SCHEMATIC DIAGRAM OF THE APPARATUS
The dotted lines indicate the direction of the principal light beams. See text.

experiment. The apertures, which determined the size of the test-fields, were inserted in the plane of the background, *A*.

During each experimental period, differential thresholds were determined by the method of limits for a series of apertures of varying size at a single intensity. The order of presentation of the field-sizes was varied for the *O*s and, in different experimental sessions, was varied for the same *O*.

Six test-areas were used. When viewed at a distance of 24 cm. from the eye they subtended visual angles of approximately 1°, 1° 11′, 1° 23′, 2° 3′, 3° 50′, and 5° 17′. The writers served as *O*s. Determinations were made at 11 intensities for the right eye of *AHH* and at 10 intensities for the left eye of *LMH*. These intensities, measured at the eye, ranged from 0.000625 ml. to 500 ml. No 'surround' illumination was used for the test-fields at the intensities 0.000625 ml. and 0.00625 ml. At the two lowest intensities, *O* was adapted to the dark for 20 min.; at all other intensities, he was adapted to the light for 4 min. A differential threshold was determined by moving the lamp $S_2$ (remotely controlled by *O* in the cubicle at *C*) until he perceived the variable test-field to be just noticeably brighter than the standard test-field. Thus all adjustments were stepwise and in the direction of increase. A com-

bination chin-support and head-rest was employed to maintain the eye at a constant distance from the plane of the background, *A*. All observations were made monocularly. The natural pupil was used; check-experiments using an artificial pupil revealed no essential change in the general form of the results.

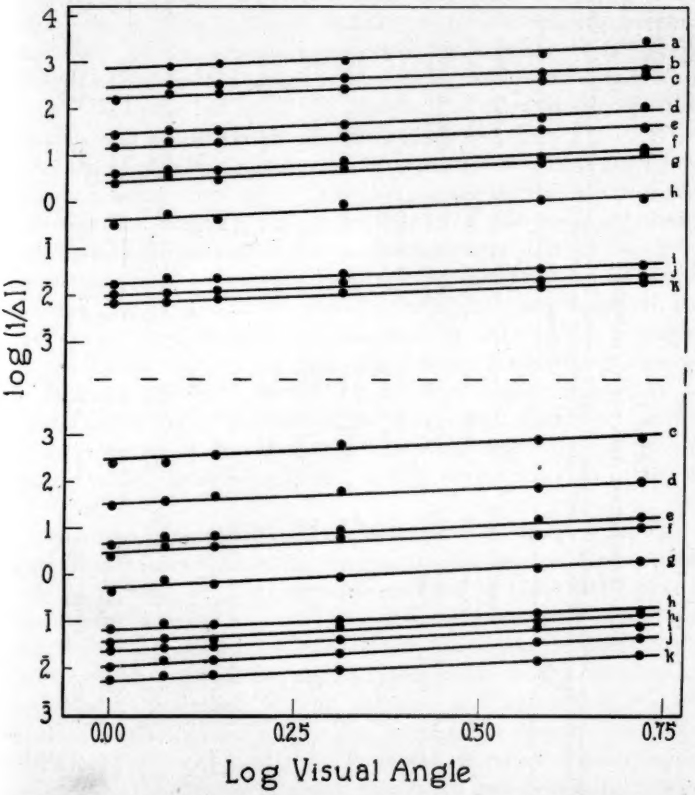


FIG. 2. VISUAL DIFFERENTIAL SENSITIVITY AS A FUNCTION OF RETINAL AREA

Two *O*s. The upper set of curves are for *AHH;* the lower set are for *LMH*. Each plotted point is an average of 10 measurements. $\Delta I$ is measured in ml.; retinal size, in degrees of visual angle. The letters associated with the curves stand for the following illuminations in milliamberts: a = 0.000625; b = 0.00625; c = 0.0125; d = 0.0625; e = 0.11; f = 0.625; g = 1.10; h = 11.0; $h_1$ = 62.5; i = 110.0; j = 250.0; k = 500.0. Each curve indicates that differential sensitivity ($1/\Delta I$) increases with an increase in retinal area, for each *O*, and for all levels of illumination. The slopes of the solid lines are identical for a given *O*, indicating that the rate of change in log ($1/\Delta I$) with respect to log (*retinal area*) is independent of illumination.

## TABLE I

### ΔI-Values for Retinal Area and Illumination

Each entry is an average of 10 monocular ΔI-measurements for the specified O. A is the area in terms of visual angle. I is the intensity of the standard test-field in ml. ΔI is the difference between the standard and the adjusted intensity in ml. $\sigma_{\Delta I}$ is the root-mean-square variation of the distribution.

| | | Visual angle | | | | | |
|---|---|---|---|---|---|---|---|
| I (in ml.) | O | 1° | 1°11′ | 1°23′ | 2°3′ | 3°50′ | 5°17′ |
| .000625 | AHH | | .00127 ± .00058 | .00110 ± .00026 | .000805 ± .00043 | .000709 ± .00057 | .000337 ± .00020 |
| .00625 | AHH | | .00296 ± .00017 | .00317 ± .00015 | .00217 ± .00014 | .00167 ± .00016 | .00143 ± .00038 |
| .0125 | AHH | .00671 ± .00039 | .00459 ± .00036 | .00490 ± .00023 | .00376 ± .00013 | .00194 ± .00018 | .00185 ± .00011 |
| | LMH | .00423 ± .00034 | .00438 ± .00025 | .00281 ± .00014 | .00154 ± .00020 | .00126 ± .00024 | .00120 ± .00017 |
| .0625 | AHH | .0363 ± .0014 | .0280 ± .0011 | .0302 ± .0012 | .00225 ± .00080 | .0153 ± .00063 | .00872 ± .00079 |
| | LMH | .0355 ± .0017 | .0281 ± .0014 | .0206 ± .0026 | .0147 ± .0013 | .0131 ± .0010 | .0100 ± .0011 |
| .11 | AHH | .0672 ± .0027 | .0488 ± .00075 | .0537 ± .0022 | .0403 ± .0012 | .0273 ± .0014 | .0262 ± .0025 |
| | LMH | .0381 ± .0025 | .0307 ± .0038 | .0218 ± .0017 | .0156 ± .0026 | .0144 ± .0028 | .0139 ± .0013 |
| .625 | AHH | .257 ± .010 | .202 ± .0097 | .234 ± .0062 | .124 ± .0054 | .105 ± .0055 | .0676 ± .0041 |
| | LMH | .253 ± .032 | .148 ± .0045 | .148 ± .011 | .102 ± .0059 | .0864 ± .0084 | .0524 ± .0088 |
| 1.1 | AHH | .413 ± .019 | .230 ± .014 | .370 ± .026 | .162 ± .014 | .140 ± .010 | .0704 ± .0097 |
| | LMH | .434 ± .028 | .240 ± .025 | .264 ± .022 | .143 ± .012 | .140 ± .013 | .0796 ± .0085 |
| 11.0 | AHH | 3.37 ± .160 | 1.74 ± .200 | 2.50 ± .081 | 1.03 ± .080 | .856 ± .110 | .796 ± .094 |
| | LMH | 2.44 ± .21 | 1.28 ± .18 | 1.64 ± .16 | 1.01 ± .07 | .625 ± .13 | .472 ± .08 |
| 62.5 | LMH | 15.7 ± .74 | 10.7 ± .76 | 11.7 ± .62 | 9.14 ± .65 | 7.28 ± .57 | 5.94 ± .57 |
| 110.0 | AHH | 62.3 ± 2.8 | 42.4 ± 1.8 | 46.0 ± 3.0 | 33.3 ± 1.6 | 26.6 ± .32 | 22.2 ± 1.3 |
| | LMH | 34.0 ± 1.6 | 24.7 ± 2.8 | 26.2 ± 2.3 | 11.6 ± 1.7 | 9.96 ± 1.5 | 6.64 ± 9.0 |
| 250.0 | AHH | 90.1 ± 2.6 | 87.4 ± 2.8 | 79.1 ± 3.3 | 51.0 ± 4.3 | 46.8 ± 3.3 | 40.6 ± 5.8 |
| | LMH | 92.9 ± 6.8 | 63.7 ± 7.1 | 65.8 ± 6.2 | 48.9 ± 2.7 | 37.5 ± 3.9 | 27.1 ± 3.6 |
| 500.0 | AHH | 157.0 ± 12.0 | 137.0 ± 2.2 | 104.0 ± 5.3 | 85.3 ± 4.4 | 64.7 ± 3.4 | 46.4 ± 3.6 |
| | LMH | 184.0 ± 12.0 | 144.0 ± 6.4 | 133.0 ± 8.4 | 108.0 ± 5.6 | 100.0 ± 6.7 | 49.4 ± 3.9 |

*Results.* The results are presented in Table I. Each $\Delta I$-value, in millilamberts (ml.), is the average of 10 measurements; $\sigma_{I}$ is the root-mean-square deviation of a single observation.[2] At any given intensity-level, in general, the $\Delta I$-values for every observer tend to *decrease* as the retinal area *increases*. This finding is consistent with, and definitely confirms, the work of the previously mentioned investigators (*vide supra*). The general nature of the relationship obtaining between differential sensitivity for brilliance and retinal size, however, may be perceived more easily by examining the data graphically. See Fig. 2.

In this figure, the logarithm of $1/\Delta I$ is plotted for each $O$ against the logarithm of the visual angle (in degrees) at the specified levels of illumination. Examination of the functions for *AHH* (the upper set) shows, in general, that visual differential sensitivity increases with the size of the visual angle and therefore with an increase in retinal area. The differential sensitivity functions for *LMH* exhibit essentially the same tendencies. For each $O$, and for every level of illumination, log $(1/\Delta I)$ tends in general to vary directly with the size of the visual angle, in the manner depicted in the figure.[3] Moreover, for the angular values actually employed in the present experiment, the logarithm of the visual angle is a rectilinear function of the logarithm of its tangent. Fig. 2 thus also depicts the form of the relation existing between log $(1/\Delta I)$ and log (*retinal area*).

*Discussion.* Our results indicate clearly that $1/\Delta I$ tends in general to increase as the size of the retinal image is made to increase, for any given level of illumination. The interpretation offered by Heinz and Lippay is consistent with this experimental fact: differential sensitivity does in this instance *increase* with the number of excited physiological elements.[4] On the other hand, it is also a fact that, for a given retinal area, $1/\Delta I$ varies inversely with illumination (flux per unit area) and hence *decreases* as the number of excited elements is made to increase. The assertion that differential sensitivity varies directly with the number of excited elements, retinal or neural,[5] is therefore not of general validity and must be rejected.

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[2] The exact relation existing between $\sigma/_{\Delta I}$ and $\Delta I$ has an important bearing upon the general problem of experimental 'error' *vs.* organic variability, and will be discussed elsewhere.

[3] The reliability of the departures from the general rule in the region of 1°–1°23′ is now being investigated.

[4] Heinz and Lippay, *op. cit.*, 446-447.

[5] E. M. Lowry, *op. cit.* Lowry's results indicate that $\Delta I$ is probably *smaller* for binocular discrimination than it is for monocular discrimination. This experiment (a) creates a strong presumption that the physiological processes critically responsible for the discrimination of a just noticeable difference in brilliance is located in the

In order to subsume these data under a single explanatory theory, we propose to employ what in general may be called the 'availability principle.' According to this principle, the magnitude of $1/\Delta I$ is to be correlated, not with the excitation itself, but with its *complement.*[6] This is to say that differential sensitivity is directly determined by the excitation potentially available for the discrimination of a just noticeable difference in brilliance (subjective brightness). If greater specificity is needed, then we may confine our definition of 'excitation' solely to the activities of the individual nerve fibers concerned.[7]

Consider, for instance, the case in which a uniformly illuminated, foveally located, circular light-image subtends an angle of 1° at the eye. There is a limit to the number of nerve fibers which can be excited by this image as its illumination is sufficiently increased. This limit, however, only partly defines the potentially available excitation. The absolute refractory period limits the maximal frequency for each of these fibers, and thus places a further restriction upon the total excitation potentially available. The *product* of these two limits, however, provides us with a precise neurophysiological definition of what we mean by the 'total excitation potentially available' under these conditions. Let us denote this product by the symbol, $N_P$.

Now suppose this retinal image to be monochromatically illuminated with an intensity which is above the stimulus-threshold of the *O*, again for these conditions. Corresponding to this intensity, there will be a neural excitation, $N_E$. The difference between $N_P$ and $N_E$ at any moment defines, *i.e.* is equal to, the excitation potentially available at that moment. This difference may be written in the form of a neurophysiological equation,

$$N_P - N_E = N_A \qquad [A]$$

where $N_A$ is the potentially available excitation. Relative to $N_P$, $N_A$ and $N_E$ are complementary values.

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central nervous system, and (b) is consistent with the quantitatively established fact that a light source is usually subjectively brighter when viewed binocularly than it is when viewed monocularly. See also, R. J. Lythgoe and L. R. Phillips, Binocular summation during dark adaptation, *J. Physiol.*, 91, 1938, 427-436.

[6] A. H. Holway and L. M. Hurvich, On the discrimination of minimal differences in weight: I. A theory of differential sensitivity, *J. Psychol.*, 4, 1937, 309-332.

[7] A. H. Holway, J. E. Smith, and M. J. Zigler, On the discrimination of minimal differences in weight: II. Number of available elements as variant, *J. Exper. Psychol.*, 20, 1937, 371-380, III. The rôle of frequency, *ibid.*, 21, 1937, 423-432. M. Upton and A. H. Holway, On the psychophysics of hearing: I. Monaural differential sensitivity and exposure-time, *Proc. Nat. Acad. Sci.*, 23, 1937, 29-32, On the psychophysics of hearing: II. Binaural differential sensitivity and exposure-time, *ibid.*, 23, 1937, 32-34; A. H. Holway and W. J. Crozier, Differential sensitivity for somesthetic pressure, *Psychol. Rec.*, 1, 1937, 170-176; The significance of area for differential sensitivity in somesthetic pressure, *ibid.*, 1, 1937, 178-184.

$N_P$ will probably fluctuate, depending upon the varying condition of the organism. At any moment however, $N_P$ may be regarded as constant for the light-image just described. As the intensity of this image is increased, $N_E$ will increase, and $N_A$ will therefore decrease. The results of our experiment (cf. Table I) demonstrate that $1/\Delta I$ also decreases as the intensity of such an image increases. Differential sensitivity is thus seen to vary directly with $N_A$.

We may now double the area of our circular image without displacing its center or changing its shape. If there is a point-for-point continuation of the fibers innervating foveal elements (an assumption frequently made),[8] if the retinal elements in the fovea are equally distributed (an assumption also frequently made), and if the fibers additionally available have the same average refractory period as those originally involved, then for a given level of illumination $N_P$ and $N_E$ have also been *doubled*. Hence, relative to the image which subtends an angle of one degree,

$$2N_P - 2N_E = 2N_A \qquad [B]$$

The potentially available excitation $N_A$ has also been *doubled*, and for that reason we may expect to find that $1/\Delta I$, too, has been increased. For larger areas, the neural architecture becomes increasingly complex and added oversimplification would serve no useful purpose. Nevertheless, with a sufficient increase in area, $N_A$ would probably be increased. According to the availability principle, $1/\Delta I$ would then also be expected additionally to increase. Fig. 2 shows definitely the manner in which $1/\Delta I$ actually does increase for our *Os* as the size of such a retinal image is enlarged, at ten different levels of illumination.[9]

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[8] K. S. Lashley, The mechanism of vision: II. The influence of cerebral lesions upon the threshold of discrimination for brightness in the rat, *J. Genet. Psychol.*, 27, 1930, 461-480, The mechanism of vision: V. The structure and image forming power of the rat's eye, *J. Comp. Psychol.*, 13, 1932, 173-200.

[9] Neurophysiological equations of this sort are of fundamental importance as regards (a) the construction and interpretation of sensory scales and (b) the quantitative expression of inferred nervous activities in terms of physical (c-g-s) dimensions. For example, given the specific relation between visual brilliance and the intensity of our 1° retinal image, $B = \varphi(I)$, and given the specific relation between $B$ and $N_E$, say $B = N_E$, we may with the aid of Equation [A] then determine *exactly* the relation between $1\Delta I$ and $N_A$. For the 1°-image, $N_P =$ a constant, $K$; and thus for our illustrative conditions, $K - \varphi(I) = N_A$. Since the relation between $1/\Delta I$ and $I$ is known (by experiment), the exact relation between $N_A$ and $1/\Delta I$ can be determined, analytically or graphically. These relations have already been worked out graphically for vision, for audition, for kinesthesis, and for somesthetic pressure. Reports which consider the significance of these relations for a general theory of sensory intensity will soon be available.

SUMMARY

The relationship between differential sensitivity ($= 1/\Delta I$) and the size of the retinal image has been experimentially determined at 10 levels of illumination for two *O*s. In general, $1/\Delta I$ was found to vary directly with the size of the retinal image, and to vary inversely with the illumination. These results can not consistently be accounted for simply in terms of retinal or neural excitation. For a retinal image of 'constant' size, $\Delta I$ *increases* with illumination and thus with an increase in excitation; for a constant level of illumination, on the other hand, $\Delta I$ *decreases* with an increase in the size of the retinal image, and thus varies inversely with the resulting excitation.

The data can, however, be subsumed under a single, dynamically conceived, explanatory principle: differential sensitivity varies directly with the total excitation potentially *available* for the discrimination of a just noticeable difference in brightness. This theory is fully coherent with the established facts concerning the activities of individual nerve fibers.

# THE VARYING FUNCTION OF AFFECTIVELY TONED OLFACTORY, VISUAL AND AUDITORY CUES IN RECALL

By MARGARET M. RATLIFF, University of Kentucky

Discrepancies in results of investigations studying the relation of affective tone to memory have been attributed to the faulty techniques employed. Meltzer, in a review of studies conducted prior to 1930, emphasized the inadequacies in methods used and the disagreement in findings.[1] In contrast, better controlled experiments made since that time tend to agree in confirming Freud's concept as to the hedonistic selectivity of memory.[2]

These discrepancies in experimental findings are in part a consequence of assuming rather than determining the affective tone of the material, of using stimuli of differing degrees of familiarity, of directions given, and of gross methods of measurement. In a series of studies reported by White and others,[3] these factors were controlled by using stimuli judged as *P* (pleasant) and *U* (unpleasant) by the *S*s. The degree of familiarity was controlled by using stimuli equated for number of associates. The method of measurement was refined by treating results in terms of reaction-time as well as number of responses made. After isolation of these and other factors, the resultant superiority of *P* material in recall and reaction-time has been interpreted as supporting the Freudian theory of repression.

The concept of repression has been studied experimentally for the greater part by the use of symbolic material, but if this theory is correct as commonly stated, stimuli arousing *U* sensations as well as *U* ideas should be forgotten. Also, there should be an associative spread to stimuli linked with the inhibited material since one of the principles of repression is that not only the *U* experiences themselves but the systems of ideas connected with them are inhibited. The present study attempts to test these propositions by measuring the comparative recall and reaction-time of

* Accepted for publication April 25, 1938. The writer wishes to acknowledge her indebtedness to Professor M. M. White, under whose direction this study was made.

[1] H. Meltzer, The present status of experimental studies of the relationship of feeling to memory, *Psychol. Rev.*, 37, 1930, 127-139.

[2] G. M. Gilbert, The new status of experimental studies on the relationship of feeling to memory, *Psychol. Bull.*, 35, 1938, 26-35.

[3] M. M. White, Some factors influencing recall of pleasant and unpleasant words, this JOURNAL, 48, 1936, 134-139; M. M. White and M. M. Ratliff, The relation of affective tone to the learning and recall of words, *ibid.*, 46, 1934, 92-98; M. M. White and M. Powell, The differential reaction-time for pleasant and unpleasant words, *ibid.*, 48, 1936, 126-133.

numbers combined by the paired-associate method with affectively toned auditory, visual, and olfactory stimuli.

*Procedure.* Hartman has arranged the numbers from one through 99 in order of ease of recall.[4] Numbers thus equated for difficulty were taken from his list and combined with *P* and *U* stimuli. The numbers paired with *P* material for one *S* were combined with *U* stimuli for the next in order that every number might appear an equal number of times with *P* and *U* cues. It is contended that any difference in the total recall of these numbers for the group as a whole would be a function of the influence of the sensory cues with which they were associated.

Griffitts,[5] Stagner,[6] and White,[7] found that *P* words have more associates than *U* words. The material in this study was, therefore, equated by means of a free association test of 30 sec. for every stimulus. In order to assure the affective tone, the *S*s rated the stimulus as *P*, *U*, or *I* (indifferent) twice immediately prior to each experiment. On the basis of this information 10 stimuli equated for number of associates were selected for every *S*; 5 of these stimuli were twice judged *P* and 5 were twice judged *U*. For a few *S*s it was not possible to find 10 stimuli meeting these requirements and in these instances the number was reduced to 8 with 4 in each affective category. The results for *S*s with fewer pairs to learn were similar to those for *S*s with longer lists. A third affective rating was taken at the close of each experiment and the shifts in rating were so few that the findings were not changed appreciably. The changes in rating from *P* at the beginning of the experiment to *U* at the end were 1%, 2%, and 3% for the group as a whole for olfaction, vision and audition, respectively. In the same order, the shifts from *U* to *P* ratings were 2%, 3%, and 1%, respectively.

The colors were similar to those in the Milton Bradley series and were shown as 3 x 6 in. rectangles with 1-in. borders. The odors included oils of rose, anise, pine, bergamot, juniper, origanum, eucalyptus, tansy, thyme, citronella, sassafras, lavender, neroli, cedarwood, cajeput, cassia, and others. The consonant chords were composed of the root, third, fifth, and octave intervals. The dissonant chords contained at least two tones foreign to the chord, such as the second, ninth, or eleventh intervals. The chords were limited to the middle range of the keyboard.[8]

*Presentation.* The experimental situation was the same for all three parts of the study, except as it was varied by the sensory cues. The colors were exposed through a window in the wall separating the experimental room from that of *S*'s. The sounds were amplified from phonograph records composed of four-interval chords played on the Hammond electric organ. The amplifier was located on the wall 3 ft. above the tachistoscope. The olfactory stimuli were presented by means of a pressure apparatus which forced air from bottles containing the oils into a glass tube extending into *S*'s room and terminating at the level of the voice-key. The *S*s were instructed to inhale immediately after hearing the ready signal. Electric fans assisted in the

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[4] George W. Hartman, Determination of easy and difficult number combinations by immediate auditory memory, *Proc. 9th Inter. Cong. Psychol.*, 1929, 210.

[5] C. H. Griffitts, Results of some experiments on affection, distribution of association and recall, *J. Exper. Psychol.*, 3, 1920, 447-464.

[6] Ross Stagner, The redintegration of pleasant and unpleasant experiences, this JOURNAL, 43, 1931, 463-468.

[7] White, *op. cit.*

[8] The writer wishes to thank Mr. Alexander Capurso for his assistance in composition of the chords.

ventilation of the room and no detectable odors persisted from one stimulus to the next. Approximately 45 sec. elapsed between presentations in order to avoid fatigue effects and adaptation.

The instructions also were the same for all the experiments, except as varied by the sensory cues. The series were not started until the *Ss* understood the directions and had become accustomed to speaking into the voice-key of the chronoscope.

Twenty-four *Ss* took part in the auditory experiment, 26 in the visual and 21 in the olfactory. The procedures for learning and recall were the same for all three studies. Each sensory stimulus was combined with a number and the *Ss* were instructed to learn in order that they would be able to respond with the correct number when the sensory cue alone was given. The order in which the pairs were given was determined by shuffling cards before each presentation. Instructions were to respond as quickly as possible. The *Ss* were taken individually and were seated comfortably before the tachistoscope.

The criterion for learning was 3 correct repetitions. When a number was recalled correctly, the experimenter said 'Right.' When there was an incorrect response or failure to respond, *E* said 'Wrong' and the correct number was shown. After a number had been recalled correctly twice it was dropped from the series until all the numbers had met this criterion. The entire list was then presented and learning was continued until each number had been associated correctly a third time.

Recall was taken immediately, 5 min. later, and 10 min. later. In the 5-min. intervals between recall series, the *Ss* read copies of the *Reader's Digest*. The time required to respond was measured by the Dunlap chronoscope and the number of correct responses was tallied. The *Ss* responded into the voice-key of the Dunlap apparatus, which started as the color, the chord, or the odorous air was presented. Approximately 2 sec. elapsed between the ready signal and presentation of the stimuli.

*Results.* The mean recall of numbers combined with the different sensory cues is given in Table I. It will be seen that recall was greater for numbers combined with *P* colors and chords than for numbers paired with *U* colors and chords. The reverse was found for olfaction, that is, when numbers were linked with *U* odors they were recalled more often than when they were shown with *P* odors. These findings hold for all three periods of recall in each of the three sense fields except in the immediate recall of numbers of colors, where an qual number of *P* and *U* stimuli were responded to correctly.

The results for reaction-time, given in Table II, agree with those in Table I for mean recall in showing that the speed of recall follows the same general tendencies as those for gross recall, that is, amount of recall. Mean reaction-times were less for the recall of numbers paired with *P* visual and auditory stimuli than for numbers paired with *U* colors and sounds for the three periods of recall without exception. Again the reverse is found in olfaction, where *U* odors consistently elicited quicker responses than did *P* odors for the three intervals of recall.

Statistical treatment of the data, included in Tables I and II, indicates

that the differences are small and while they would not be considered statistically significant by Fisher's criterion[9] for the limited number of cases, they are consistent for all analyses of the data.

In obtaining the mean reaction-times, only the correct responses were

TABLE I

MEAN RECALL OF NUMBERS COMBINED WITH *P* AND *U* STIMULI

| Paired stimuli | Recall | *P* | | | *U* | | | S.E. Diff. | Obt. Diff. | Crit. Ratio | Chances in 100 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Mean | S.D. | S.E. | Mean | S.D. | S.E. | | | | |
| Audition | Immed. | 2.29 | 1.02 | .21 | 1.71 | 1.10 | .23 | .29 | .58 | 2.02 | 98 |
| | 5-min. | 2.04 | 1.21 | .25 | 1.92 | 1.11 | .23 | .28 | .12 | .45 | 67 |
| | 10-min. | 2.33 | 1.28 | .27 | 1.88 | 1.33 | .28 | .32 | .46 | 1.44 | 92 |
| Vision | Immed. | 3.85 | 1.13 | .23 | 3.85 | .95 | .19 | .22 | .00 | | |
| | 5-min. | 3.77 | 1.22 | .24 | 3.62 | 1.08 | .22 | .23 | .15 | .68 | 75 |
| | 10-min. | 3.92 | .99 | .20 | 3.85 | .91 | .18 | .21 | .08 | .37 | 64 |
| Olfaction | Immed. | 2.77 | 1.13 | .25 | 2.91 | .85 | .18 | .22 | −.14 | .62 | 73 |
| | 5-min. | 3.00 | 1.09 | .24 | 3.32 | .76 | .17 | .16 | −.32 | 1.97 | 97 |
| | 10-min. | 2.86 | 1.14 | .25 | 3.18 | .89 | .19 | .22 | −.32 | 1.43 | 92 |

TABLE II

MEAN REACTION-TIME FOR RECALL OF NUMBERS COMBINED WITH *P* AND *U* STIMULI
(Times in 1/6 sec.)

| Paired stimuli | Recall | *P* | | | *U* | | | S.E. Diff. | Obt. Diff. | Crit. Ratio | Chances in 100 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Mean | S.D. | S.E. | Mean | S.D. | S.E. | | | | |
| Audition | Immed. | 13.99 | 6.48 | 1.49 | 15.48 | 7.89 | 1.81 | 1.47 | −1.49 | 1.02 | 84 |
| | 5-min. | 15.82 | 8.10 | 1.86 | 17.07 | 7.91 | 1.82 | 1.44 | −1.25 | .87 | 80 |
| | 10-min. | 16.40 | 8.56 | 2.08 | 16.96 | 8.73 | 2.12 | 1.50 | −.56 | .37 | 64 |
| Vision | Immed. | 9.62 | 1.95 | .39 | 10.75 | 3.56 | .71 | .72 | −1.13 | 1.57 | 93 |
| | 5-min. | 8.53 | 1.82 | .36 | 10.53 | 4.74 | .95 | 1.01 | −1.99 | 1.97 | 97 |
| | 10-min. | 8.47 | 1.85 | .37 | 10.47 | 3.55 | .67 | .72 | −1.99 | 2.76 | 100 |
| Olfaction | Immed. | 26.99 | 12.50 | 2.73 | 22.09 | 6.75 | 1.47 | 2.22 | +4.91 | 2.21 | 99 |
| | 5-min. | 26.49 | 11.36 | 2.48 | 25.46 | 10.36 | 2.26 | 2.72 | +1.03 | .38 | 64 |
| | 10-min. | 25.81 | 9.56 | 2.14 | 23.28 | 8.51 | 1.90 | 2.37 | +2.53 | 1.07 | 85 |

included as information concerning the number of errors and their affective tone is considered independently. Arbitrarily assigning a value to the forgotten numbers might have distorted the means unnecessarily.

Analysis of incorrect responses, presented in Table III, shows that more errors were made in response to *U* auditory and visual cues than to *P* stimuli in these sense fields. In olfaction there were more errors in the recall of numbers combined with *P* than with *U* odors. The only exception is found in the immediate response to colors where the number of

[9] J. P. Guilford, *Psychometric Methods*, 1936, 62.

errors was the same since an equal number of *P* and *U* responses was made.

Table IV gives the number of *Ss* whose recalls and reaction-times for the *P* paired-associates exceed, were equal to, or were less than those

TABLE III

COMPARISON OF ERRORS IN RECALL

| Paired stimuli | Total errors in recall | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Immed. | | | 5-min. | | | 10-min. | | |
| | *P* | *U* | diff. | *P* | *U* | diff. | *P* | *U* | diff. |
| Audition | 61 | 75 | −14 | 67 | 70 | −3 | 60 | 61 | −11 |
| Vision | 18 | 18 | 0 | 20 | 24 | −4 | 16 | 18 | − 2 |
| Olfaction | 28 | 25 | 3 | 23 | 16 | 7 | 26 | 19 | 7 |

TABLE IV

NUMBER OF *Ss* WHOSE RECALLS AND REACTION-TIMES FOR THE *P* PAIRED-ASSOCIATES EXCEEDED, WERE LESS THAN, AND WERE EQUAL TO THOSE WITH THE *U* ASSOCIATES

| | Paired stimuli | Immed. | | | 5-min. | | | 10-min. | | | Combined | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | *P*>*U* | *P*<*U* | *P*=*U* | *P*>*U* | *P*<*U* | *P*=*U* | *P*>*U* | *P*<*U* | *P*=*U* | *P*>*U* | *P*<*U* | *P*=*U* |
| Recall | Audition | 13 | 5 | 6 | 10 | 7 | 7 | 14 | 7 | 3 | 11 | 9 | 4 |
| | Vision | 6 | 6 | 14 | 9 | 5 | 12 | 6 | 5 | 15 | 12 | 6 | 8 |
| | Olfaction | 6 | 8 | 8 | 6 | 11 | 5 | 4 | 9 | 9 | 6 | 13 | 3 |
| Reaction-time | Audition | 12 | 8 | 4 | 13 | 7 | 4 | 11 | 7 | 2 | 13 | 9 | 2 |
| | Vision | 17 | 9 | | 17 | 9 | | 18 | 8 | | 18 | 8 | |
| | Olfaction | 6 | 16 | | 8 | 14 | | 8 | 12 | 2 | 5 | 17 | |

TABLE V

AVERAGE NUMBER OF REPETITIONS REQUIRED TO ESTABLISH THE FIRST PAIRED-ASSOCIATION AND THE AVERAGE NUMBER REQUIRED TO LEARN THE COMPLETE SERIES

| | Paired stimuli | *P* | | | *U* | | | S.E. Diff. | Obt. Diff. | Crit. Ratio | Chances in 100 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Mean | S.D. | S.E. | Mean | S.D. | S.E. | | | | |
| First association | Audition | 3.57 | 2.87 | .27 | 3.72 | 2.94 | .27 | .38 | −.15 | .38 | 64 |
| | Vision | 2.18 | 1.39 | .13 | 2.28 | 1.36 | .13 | .18 | −.10 | .57 | 71 |
| | Olfaction | 2.50 | 2.23 | .24 | 2.36 | 2.05 | .22 | .32 | .13 | .42 | 65 |
| Complete series | Audition | 17.25 | 7.18 | 1.50 | 17.96 | 6.35 | 1.32 | 1.40 | −.71 | .51 | 69 |
| | Vision | 9.88 | 4.39 | .88 | 10.35 | 4.05 | .81 | .97 | −.46 | .48 | 68 |
| | Olfaction | 10.09 | 6.92 | 1.51 | 9.55 | 5.84 | 1.28 | 1.43 | .55 | .38 | 64 |

with the *U* associates. It will be observed from this table that more *Ss*, in audition and vision, recalled more *P* than *U* paired associates (11-9 in audition and 12-6 in vision), and that in olfaction the results are quite the other way—more *Ss* recalled *U* (13) than *P* (6) paired-associates. The reaction-times of the *Ss* reports varied similarly: more *Ss* gave shorter reaction-times in audition and vision for *P* paired-associates than for *U* (13-9 and 18-8, respectively), whereas in olfaction more *Ss* gave shorter time for *U* (17) than for *P* (5) paired-associates.

Table V gives the average number of repetitions required to establish the first association between numbers and *P* and *U* sensory cues and to learn the complete series. Learning was more rapid, as these results reveal, for numbers combined with *P* than with *U* colors and sounds. Numbers combined with *U* odors, however, were learned more quickly than those combined with *P* odors.

*Discussion.* According to the central theory of emotions as advanced by Head,[10] Dana,[11] Cannon,[12] and Bard,[13] the neural mechanisms responsible for emotional expression are located at the base of the thalamic region. It is also thought that the cortex exerts an inhibitory control over these expressions. In a critical review of this theory, Lashley agrees in localizing the motor centers for emotional expression in the hypothalamus but raises doubts as to the source of inhibition.[14] Although evidence of emotional hyperexcitability in decorticate animals lends support to the hypothesis that the cortex inhibits the rage reaction, he does not believe that clinical evidence justifies the postulation of the cortex as the only source of inhibition. In conclusion, he states that, "although normal inhibition of the thalamic centers for expressive movement is indicated, there is some uncertainty as to the source of inhibition."[15]

Previous to reading the article by Lashley, quoted above, it was suggested by the writer that results of the present experiment might be interpreted as indicating that the repressing mechanism for *U* stimuli, as postulated by the Freudians, is represented in the thalamus and therefore, operates in audition and vision but not in olfaction.[16] This position is taken because neurologists locate the lower centers for audition and vision in the diencephalon and those for olfaction in the rhinencephalon. If, however, the cortex is found to be the center for repression and if it exerts a greater and more direct control over the thalamus than over the uncus and hippocampus, the difference between the auditory-visual inhibitions and those of olfaction might be accounted for.

It is known that the olfactory portions of the brain are phylogenetically older than those of vision and audition. It may be that affective values in olfaction are more constant for the individual ontogenetically and the species phylogenetically than are those for audition and vision. It is possible that at one time in the development of man, *U* colors, sounds, and odors alike may have been recalled more readily than *P* stimuli due to the exigencies of survival. With modification in the

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[10] H. Head, *Studies in Neurology*, 1920, Vols. 1-2, 1-862.

[11] C. L. Dana, The anatomic seat of emotions, *Arch. Neurol. & Psychiat.*, 6, 1921, 634-639.

[12] W. B. Cannon, The James-Lange theory of emotions; a critical examination and an alternative theory, this JOURNAL, 39, 1927, 106-124; Again the James-Lange and the thalamic theories of emotion, *Psychol. Rev.*, 38, 1931, 281-295.

[13] P. Bard, On emotional expression after decortication with some remarks on theoretical views, *Psychol. Rev.*, 41, 1934, 309-329, 424-429; The neuro-humoral basis of emotional reactions, in *A Handbook of General Experimental Psychology*, 1934, 264-311.

[14] K. S. Lashley, The thalamus and emotion, *Psychol. Rev.*, 45, 1938, 42-61.

[15] *Op. cit.*, 47.

[16] M. M. Ratliff, The varying function of affectively toned olfactory, visual and auditory cues in recall, Thesis, University of Kentucky, 1937.

neural organization there may have been an accompanying modification in the behavior patterns.

There would seem to be less constancy in life situations in affective judgments of color and sounds than of odors. Although individual and cultural differences are large in all three sense fields, the preferences for color and harmony combinations show considerable variation as evidenced by the contemporary preference for the harmonics of Stravinsky and other modern composers over those of Johann Strauss of another generation. Color combinations once thought unpleasant have become acceptable. In contrast, affective judgments of odors seem to be relatively constant for the individual. It would be interesting to test these hypotheses in the gustatory quale and to determine the relative efficiency in recall of *P* and *U* stimuli.

Data obtained in these studies might be explained in terms of the striking quality of the stimuli or some other factor of advantage. For example, the striking quality of the *U* olfactory stimuli may have been greater than that of the *P* odors. If this reasoning is followed it necessitates consideration of the striking quality of the *P* and *U* sounds and colors. Introspective estimates of the stimulus values were asked of a number of *S*s and it was reported that the striking quality seemed to be relatively the same for *P* and *U* colors but that *U* sounds and odors seemed to stand out more than those of *P* stimuli in these fields. If these findings are established, the assumption of differences in striking quality as an explanatory concept would be invalidated, as *U* sounds were inhibited although they may have been more striking in quality than *P* chords, and *U* colors were repressed although there was no reported difference in striking quality of the *P* and *U* stimuli.

*Summary.* Numbers were combined by the paired-associate method with *P* and *U* sounds, colors, and odors with instructions to learn so that when the sensory cue alone was given the correct number would be recalled. Data are based on immediate, 5- and 10-min. delayed recall analyzed in terms of amount and speed of recall, as measured by number of correct responses and reaction-time.

The results are as follows: (1) Recall was greater and reaction-time was less for numbers paired with *P* colors and sounds than for numbers combined with *U* colors and sounds. In olfaction, recall was greater and reaction-time was less for numbers paired with *U* than with *P* odors. (2) More *S*s recalled more numbers paired with *P* than with *U* colors and sounds and recalled them in shorter times. In olfaction, on the other hand, more *S*s recalled more numbers linked with *U* than with *P* odors and in shorter reaction-times. (3) Learning was more rapid for numbers combined with *P* colors and sounds than for numbers paired with *U* colors and sounds. Numbers with *U* odors were learned more rapidly than numbers with *P* odors.

The results of this study lend some support to a theory of repression for *U* material in the auditory and visual sense fields, but not in the olfactory quale. Explanation of the results obtained may be attempted on three levels; namely, the neurological, the phylogenetic, and the stimulus-value.

# FUNCTION-FLUCTUATION AND PERSONALITY TRENDS OF NORMAL SUBJECTS

By C. O. WEBER, Wells College

No doubt the most important of recent advances in the treatment of schizophrenia is the so-called "sugar-shock" method involving the injection of large doses of insulin. So far, however, the apparent success of this treatment has yielded no clear cut information regarding the nature of the disease. As regards the psychological analysis of schizophrenia perhaps the most suggestive approach is embodied in several studies which demonstrate the high day-to-day variability in output of schizophrenics. This high variability of schizophrenics is perhaps an implication of the classic symptom of "intra-psychic ataxia" (Stransky) attributed to subjects having this disease. As early as 1909, Gatewood found that schizophrenics would double or halve previous outputs when retested.[1] Hunt and Guilford studied the fluctuations of the Wheatstone cube for 25 normal, 24 dementia praecox, and 19 manic-depressives.[2] Although the schizophrenics resembled normals in average rates and dispersions of the fluctuations, they contrasted sharply with the manic depressives, showing from 4 to 6 times the number of flucuations typical of the latter cases.

Our own study follows the lines of the later experiment of Hunt's who compared 8 schizophrenic patients, 5 paretics and 8 controls as regards variability of output on 4 tests each one of which was administered 15 times with intervals of about 5 days between tests.[3] Hunt's study also contains results on the introspective states of the subjects during examination. As regards the variability scores, Hunt found the schizophrenics much more variable than the normal controls on all tests, the paretics holding an intermediate position. It would have been interesting to know the relations of the differences found by Hunt to the standard errors of the differences. The differences found, however, are so large that in all probability they exceed the measures of error.

Our own study was with 44 college freshmen. The traditional belief

* Accepted for publication March 9, 1938.

[1] L. C. Gatewood, An experimental study of dementia praecox, *Psychol. Monog.*, 11, 1909 (no. 45), 1-71.

[2] J. McV. Hunt and J. P. Guilford, Fluctuation of an ambiguous figure in manic depressive and dementia praecox patients, *J. Abn. & Soc. Psychol.*, 27, 1933, 443-452.

[3] J. McV. Hunt, Psychological government and the high variability of schizophrenic patients, this JOURNAL, 48, 1936, 64-81.

that normal introverts have something in common with schizophrenics is perhaps a bit forlorn in view of many adverse criticisms. Wholly apart from this hypothesis, however, it should be of significance to learn whether or not there is any association between variability and the personality traits of normal *Ss*.

*Subjects.* It should be stated that our 44 *Ss*, freshmen women at Wells College, were not 'average' since they were cases selected for remedial reading exercises because of their low scores on a test of reading speed and comprehension. This factor of selection, however, was shown to have little relation to the personality traits here under investigation.

*Method.* As measures of traits we had available the 'social introversion' and 'emotionality' scores of the Guilford S. E. M. Test,[4] scores of the Allport A. S. Reaction Study, and of the American Council on Education Psychological Examination.

Our variability ($V$) scores are derived from 5 tests administered to all subjects once per week for 6 successive weeks. In this respect, Hunt's study is superior to ours, since he made 15 retests. Our study, however, compensates for this defect since it involves 44 cases compared with 21 cases studied by Hunt. Hunt's 4 tests were adding, dealing cards, sorting cards, and a substitution test. It will be noted that the last 3 of these stress motor function to a marked extent. Although all of our tests involve a motor factor, each one in addition taps several mental capacities. Test 1 consists in crossing out specified numbers from a page of digits. Test 2 is a digit-letter substitution test. Test 3 is a test of perserveration trends involving the alternate printing of letters and digits. Test 4 is a test of immediate recognition memory of nonsense syllables. Test 5 consists of a series of number-completion problems increasing in difficulty.

The output on each test was scored for speed and for accuracy. The day to day variation in accuracy was in general too small to serve as suitable data for a study of individual differences. The studies which follow refer to the speed scores only.

*Measure of variability.* Hunt's coefficients of variability were computed from the formula: $V = [1000\ (\text{sum}\ \Delta - I)]\ /\ [X\ (n - 1)]$, where $\Delta$ is the difference between successive pairs of scores, $I$ the amount of improvement determined by subtracting the average of the first 2 from the last 2 scores, $X$ is the average score for all tests, and $n - 1$ is the number of score differences obtainable (which is 14 in the 15 tests given).

This formula is in general a good one, but in applying it to our results it was noted that it yields a rather *approximate* measure of $V$. Thus, suppose that two *Ss*, *A* and *B*, earn 6 sucessive scores on an assumed variability test as follows:

*A.* 10, 12, 16, 20, 22, 24
*B.* 10, 12, 14, 22, 22, 24

The improvements ($I$) of the two *Ss* (as determined by the first 2 and last 2 scores), the sum $\Delta$, the average scores ($X$) are all equal for the two cases. Hence, Hunt's formula will yield the same $V$ score for both. But it is quite evident from inspection

[4] J. P. and Ruth B. Guilford, Personality factors S, E and M and their measurement, *J. Psychol.*, 2, 1936, 109-127.

that case *B* is more variable. Hunt's method of determining improvement from the first 2 and the last 2 scores is also an approximation, and might be materially altered by averaging the first 3 and the last 3 trials. Again, it will sometimes happen that the improvement will exceed the sum Δ, in which case one appears to get a variability score of less than zero!

In addition to applying Hunt's formula to our results, we devised and applied a modified formula as follows: $V = [1000 \cdot \text{sum } (\Delta - \text{Av. c.d.})] / [X\ (n - 1)]$. Where Δ again refers to the differences between successive scores but taken with regard to sign, *i.e.* if a score is larger than the one preceding it is regarded as a plus value, if less, a minus value. The Av. c.d. (Average constant deviation) is the algebraic average of the Δ's. The subtraction of the Av. c.d. from each Δ is also algebraic, but the resulting values are regarded as without sign for the purpose of securing sum Δ.

Applying this formula to cases *A* and *B* above we get the following *V* scores: for *A*, V = 5.55; for *B*, V = 12.02. Our modification of Hunt's formula has the advantage that the final *V* score is affected by every change throughout the series of successive scores, in each instance separating constant from inconstant gains and losses. If this formula is applied to scores showing constant changes either in the direction of gain or loss the resulting *V* score will be zero.

*Results.* We had computed the *V* scores of each of the 44 *S*s both by means of Hunt's formula and our own. In a tentative test of their relations to the personality test scores we formed contrasting groups of introverts and extroverts, *S*s of low and of high emotionality, of ascendant and submissive *S*s, of high and of low intelligence. The contrasts in the average *V*'s of such paired groups are quite alike in direction for Hunt's and for our own formula. The main difference is that Hunt's formula gives a much greater range of *V* scores within each group, indeed a range so great that measures of probable error tend to obliterate the score differences between the paired groups. In what follows, we confine our study of group differences in variability as determined by the new formula.

The *V* score of every *S* was determined for each one of the five sub-tests. We then secured a weighted average *V* score for every *S* by a system of weighting the *V* scores on the separate tests so that all sub-tests contributed equally to the final *V* score.

The 44 freshmen who served as *S*s in this study had been selected for an experiment in remedial reading, and all of them ranked at the 30 percentile or less in reading speed and reading comprehension on the Iowa Advanced Silent Reading Test. These cases, however, do not appear to be selected as regards the personality traits involved in the present study. This conclusion is suggested by product-moment correlations between the reading and the personality scores. None of these r's rise above the value of +0.2—in fact, in 6 out of 9 r's, measures of P.E. exceed the r's. Our

cases are therefore approximately unselected so far as reading scores are concerned. As regards intelligence score, however, they are definitely a selected group, since the *Ss* who earn low reading scores are also low on the intelligence test. The r between intelligence score and reading speed score (98 cases) is $+0.41 \pm .034$; between reading comprehension and intelligence the r is $\pm 0.73 \pm .018$. As will appear later there is no marked relationship between intelligence score and *V* score. On the whole, then, the results now to be presented regarding the relationships between

TABLE I

DIFFERENCES IN VARIABILITY OF GROUPS CONTRASTING IN PERSONALITY TRAITS

| | Trait or Trait Combination | N | V Score | D/$\sigma_{diff.}$ | Probability |
|---|---|---|---|---|---|
| 1 | High intelligence group | 20 | 67.6 | .34 | 63 |
| | Low intelligence group | 20 | 64.9 | | |
| 2 | Extrovert group | 20 | 71.5 | 1.55 | 94 |
| | Introvert group | 20 | 59.6 | | |
| 3 | Ascendant group | 20 | 75.3 | 2.32 | 99 |
| | Submissive group | 20 | 58.0 | | |
| 4 | Low emotionality group | 20 | 74.5 | 2.14 | 98 |
| | High emotionality group | 20 | 58.3 | | |
| 5 | High emotionality & high ascendancy | 8 | 65.4 | 0.16 | 57 |
| | Low emotionality & low ascendancy | 8 | 63.6 | | |
| 6 | Low emotionality & high ascendancy | 13 | 79.5 | 2.91 | 99.8 |
| | High emotionality & low ascendancy | 12 | 52.6 | | |
| 7 | Low emotionality & high ascendancy | 13 | 79.5 | 1.67 | 95 |
| | Low emotionality, low ascendancy & high emotionality, high ascendancy | 16 | 64.5 | | |

*V* and personality traits for our 44 *Ss* may be regarded as approximately valid for wholly unselected *Ss*.

For the purpose of determining the relationships between *V* scores and personality trait scores paired groups of *Ss* were formed representing the extremes of specific dimensions of personality. Since there were 44 *Ss* in all we selected 20 cases at the extremes of each distribution, always omitting the 4 cases having scores nearest the mode. The two groups of 20 *Ss* each were then compared by means of the 'critical ratio' method in which the score difference between the two groups is divided by $\sigma_{diff.}$. The results are given in Table I. Ratios 5, 6 and 7 derive from fewer cases since they involve the selection of groups on the basis of two criteria, every *S* being in the upper (or lower) quintile of each of the two criteria involved.

The last column of Table I gives the chances that the difference found

between the paired groups is a true difference on the basis of 100. The more intelligent *Ss* are more variable than those of low intelligence score, but the difference between the two groups is not significant. The c.r. (critical ratio) for the introvert-extravert groups suggests that extraverts are reliably more variable than introverts. This result is out of harmony with the studies of Hunt and others with pathological *Ss*, if, indeed, we may assume that some common factor is shared by normal introverts and schizophrenic patients. Of course, no one imagines schizophrenia to be constituted purely of extreme introversion. If we take into consideration the other traits of schizophrenics it is possible to bring our results into significant harmony with Hunt's. To this end, we call attention to the following considerations.

C.r.'s 3 and 4 indicate that ascendancy and emotionality show a closer relationship to *V* than does intro-extraversion. High variability is characteristic of ascendant *Ss* and for *Ss* showing low emotionality score. C.r's 5, 6, and 7 show the variabilities of groups formed on the basis of more than one trait. One might perhaps expect that high *V* is naturally characteristic of *Ss* who fall at the extremes of a deviation, since to fall within such an extreme is already a measure of variability. It is quite apparent, however, that not mere deviation as such, but rather the *kind* of deviation is the important factor; for *Ss* combining high emotionality and low ascendancy are the least variable of all on the special *V* tests. By contrast, the *Ss* who combine low emotionality and high ascendancy are the most variable of all on the special *V* tests, the difference between these two groups being almost perfectly reliable (probability = 99.8).

*Interpretation.* It is possible to interpret our findings in a way which shows them to be in accord with previous studies of the variability of schizophrenics, and in harmony with current views of psychiatrists regarding this disorder. That schizophrenic patients are always introvert is questioned, but it is a clinically attested fact that schizophrenia quite generally involves a decay of the emotional life—a general onset of apathy in the sphere of feeling. Normal introverts, as Guilford's test suggests, incline to be emotionally sensitive. With a group of 76 sophomores we find that social introversion gives an *r* of $+0.37$ ($\pm 0.066$) with emotionality.

The higher emotional *Ss* are destined to experience difficulties in the general sphere of social adjustment. No doubt, for an emotional *S* the pleasures of social 'felicities' may be high, if and when they are attained. But such possible pleasures are always purchased at the price of exceptional pains which an emotional person must suffer where social friction occurs. Two reactions of defense against such stresses are familiar to psychologists. One reaction is that of social withdrawal (introversion), and the other is, as Kretschmer aptly calls it, "affective cooling."[5] The

[5] E. Kretschmer, *Physique and Character*, 1925, Ch. 10.

normal introvert resorts to the first mode of compensation, by withdrawing, to a degree, from social contacts. The schizoid *S* carries this to an extreme, and in addition further reduces friction by an inner loss of responsiveness. This emotional desensitization may begin and continue as an inhibition of feeling, but in the end the result is a true 'laming' and 'cooling' of the life of feeling. Such want of feeling is apparently one common characteristic of schizophrenics and of that portion of our *Ss* who earn low emotionality scores, and who in addition share with schizophrenics the tendency to high variability.

Again, quite a number of studies show that normal introverts tend to earn high submission (non-ascendancy) scores. This was strikingly the case with our special study of 76 sophomores using the Guilford Test, giving an $r$ of $-0.70$ ($\pm 0.04$) between social introversion and ascendancy score (Allport Test). This suggests that the normal introvert, although emotionally sensitive, will suffer little stress on this account *since she is also non-agressive;* and in this particular enjoys the comparative mental calm which some pious moralists attribute, perhaps in too large a measure, to all submissiveness. The schizoid introvert, on the other hand, gives rather good evidence of ascendancy. This is especially apparent in the negativism of catatonic *Ss*, which is difficult to interpret otherwise than as an inveterate and perverse self-assertion. The negativism of catatonics has been established experimentally by Williams[6] and Bartlett.[7] Kretschmer interprets the attitudinizing of catatonic patients as a crude form of primitive self-assertion in the presence of others. In the hebephrenic form of schizophrenia we also get evidence of a lingering egotism. No small share of the coy, silly, and embarrassed behavior of these patients is described by McDougall's phrase, "He never freely asserts himself and he never freely submits." McDougall regards schizophrenia in general as characterized by marked ego-centricity combined with a marked incapacity for self-assertion due to strong inhibitions. The normal counterpart to this psychosis is embarrassment in which we also see impulses to self-assertion beset by inhibitions.[8]

In brief, our suggestion is that the common characteristic shared between schizophrenics and normal introverts is not merely introversion, for social introversion at least is a mode of behavior rather than a primary character trait—it is a result rather than a cause. Although introvert behavior is a resultant of many causes our study calls attention to two causal complexes in particular. (1) One type of introvert is highly emotional and develops social introversion as a reaction of defense, but only to a degree, since he is also submissive and on this account escapes some social frictions and suffers less from frictions which cannot be escaped. (2) The other type is also highly emotional and develops social introversion as an escape reaction, but to a greater degree since (a) he suffers more because of his egotism and assertiveness, (b) he reduces friction still further by means of a loss of emotional responsiveness. Our experimental results suggest that our second type of introvert shares with schizophrenics the further tendency to a high day to day variability of capacities.

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[6] G. W. Williams, A study of the responses of three psychotic groups to a test of suggestibility, *J. Gen. Psychol.*, 7, 1932, 302-309.

[7] M. R. Bartlett, Suggestibility in psychopathic individuals, *ibid.*, 14, 1936, 241-247.

[8] W. McDougall, *Outline of Abnormal Psychology*, 1926, 388.

## SUMMARY

Five tests measuring a variety of capacities were administered to 44 college women of freshman grade on 6 occasions separated by intervals of one week. Test-retest variability was computed by means of a modification of Hunt's formula, and the resulting $V$ scores were studied in relation to personality test scores.

*(1)* *S* of high intelligence score are more variable than those of low intelligence, but the difference is unreliably small.

*(2)* Reliably high variability differences were found in ascendant as compared with submissive *Ss* (Allport Test).

*(3)* The variability of *Ss* having low emotionality score was reliably higher than for those having high emotionality score (Guilford Test).

*(4)* The findings are related to the previous studies of Gatewood, Guilford, and Hunt showing that schizophrenic patients are characterized by a high day to day variability of capacity.

# THE COLOR PREFERENCES OF EAST INDIANS

By THOMAS R. GARTH, MATHILDE R. MOSES, and CORRINE N. ANTHONY,
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This study of the color preferences of East Indians is one of seven which have been made with the same materials and with the same method of administration and handling of the results. It is not necessary to describe the materials excepting to say that there were seven color disks of Milton Bradley standard colored papers, as red, blue, green, orange, yellow, violet, and white. These were ½ in. in diam. mounted on cards 1½ by ¾ in. There were record blanks upon which were recorded the name of the subject, his sex, age, his school, race, and the date.

In administering the experiment the *Ss* were requested to place simultaneously before them on their desks the seven colors, to arrange the colors in the order of preference beginning with the one liked best, following with the one liked next best, and so on until the color liked least of all would be last. The *Ss* were then asked to write on the record blanks the color preference sequence as it had been arranged with the actual colors. Great care was taken to allow no other person to influence the choice of any *S*, so that the record would show the actual relative 'pull' of the colors for an individual. In this study there were 1078 *Ss*—East Indians attending school and ranging in age from 5–26 yr. These were found in the schools of Bijnor, India. There were 647 males and 431 females.

Table I shows the distribution of the *Ss* by school grade.

TABLE I

DISTRIBUTION OF *Ss* BY SCHOOL GRADE

| Grade | Boys | Girls | Total |
|---|---|---|---|
| First | 61 | 52 | 113 |
| Second | 61 | 50 | 111 |
| Third | 89 | 53 | 142 |
| Fourth | 41 | 44 | 85 |
| Fifth | 67 | 85 | 152 |
| Sixth | 86 | 58 | 144 |
| Seventh | 102 | 58 | 160 |
| Eighth to twelfth | 140 | 31 | 171 |
| Total | 647 | 431 | 1,078 |

Table II is a sample of tabulation of a part of the records of the First Grade East Indian boys. Complete tables such as these for grade and sex were made for the groups. The children in the eighth and twelfth grades were taken as in one group.

Since we are unable to get the actual age of all the *Ss*, we are not able to say anything more definite regarding age than to note that these *Ss* seemed to range from 5 to 26 years of age. The difficulty lay mostly in getting the ages of the older children. There were, however, 6-yr. and 7-yr. olds to be found in the first through the sixth grades. One 9-yr. old boy was found in the seventh grade. Nevertheless, the data for ages, as loosely judged from the imperfect data, seem to run for the grades as they do in America.

* Accepted for publication March 10, 1938.

The data were handled after the order of merit method with a view to obtaining color preference scales. These scales are derived in the same way as the Thorndike Handwriting[1] and Hillegas Composition Scales.[2] Table III shows the color preference scale for all our *Ss*. Table V gives the scales for the several grade groups. For a description of color preference scales and their derivation, see the discussion by the senior author.[3]

The sequence of color preferences for the 1078 *Ss* of this study is fairly representative of an East Indian population with its racial, environmental and cultural

TABLE II

SAMPLE RECORDS OF SOME FIRST GRADE EAST INDIAN BOYS

| | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Subjects: | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 |
| Age in yr.: | | 5 | 5 | 5 | 5 | 6 | 6 | 6 | 6 | 7 | 7 | 7 | 8 | 8 | 8 | 9 | 9 |
| Color rank | Blue | 4 | 7 | 6 | 4 | 4 | 3 | 2 | 2 | 3 | 6 | 4 | 1 | 4 | 1 | 3 | 3 |
| | Green | 3 | 6 | 2 | 2 | 1 | 5 | 5 | 4 | 4 | 2 | 2 | 3 | 5 | 4 | 2 | 6 |
| | Orange | 7 | 5 | 1 | 5 | 3 | 7 | 7 | 5 | 1 | 3 | 6 | 6 | 3 | 6 | 6 | 4 |
| | Red | 1 | 2 | 4 | 3 | 5 | 2 | 1 | 1 | 2 | 4 | 1 | 2 | 2 | 2 | 5 | 5 |
| | Violet | 2 | 4 | 5 | 6 | 2 | 4 | 6 | 6 | 5 | 5 | 3 | 4 | 1 | 3 | 4 | 1 |
| | White | 6 | 3 | 7 | 7 | 7 | 1 | 4 | 7 | 7 | 7 | 7 | 7 | 7 | 7 | 1 | 7 |
| | Yellow | 5 | 1 | 3 | 1 | 6 | 6 | 3 | 3 | 6 | 1 | 5 | 5 | 6 | 5 | 7 | 2 |

factors as they are influenced by English and American education. It will be seen that for the total group the most preferred color is Red with a value of 230 which represents the extreme length of the scale. The color least esteemed is White which falls at zero on the scale. The value of Yellow is 95, so that Red may be

TABLE III

COLOR PREFERENCE SCALE FOR THE TOTAL GROUP OF 1,078 EAST INDIAN CHILDREN AND FOR BOYS AND GIRLS

| Group (1,078) | | Boys (647) | | Girls (431) | |
|---|---|---|---|---|---|
| Color | Scale value | Color | Scale value | Color | Scale value |
| White | 0 | White | 0 | White | 0 |
| Yellow | 95 | Yellow | 114 | Yellow | 69 |
| Orange | 160 | Orange | 167 | Violet | 143 |
| Violet | 167 | Violet | 189 | Orange | 154 |
| Blue | 189 | Blue | 223 | Blue | 165 |
| Green | 204 | Green | 227 | Green | 195 |
| Red | 230 | Red | 272 | Red | 195 |

said to 'pull' about 2½ times as hard as Yellow. Blue, value 189, is almost one unit above Yellow. Orange and Violet have about the same values, 160 and 167 respectively. Green, value 204, is only slightly less preferred than Red.

As to the scales of the boys and girls the sequence may be legitimately compared, but not the scale values since the numbers involved are so disparate. At least we may say that the sequence of the colors is quite similar for the two scales excepting for the difference in Violet and Orange, Violet being preferred to Orange by

[1] E. L. Thorndike, *A Scale for Measuring the Handwriting of Children*, 1912.
[2] M. B. Hillegas, *A Scale for Measurement of English Composition by Young People*, 1912.
[3] T. G. Garth, *Race Psychology*, 1931, 1-260.

the boys and Orange to Violet by the girls, the girls being here slightly confused since the scale values are almost the same for these two colors. The boys have a longer scale, but show much confusion in their feeling for Blue and Green, the values being 223 and 227. For the girls, Red and Green have the same value, 195. Both sexes, however, agree in assigning to White a zero value. We may make a further comparison of the scales for the sexes by noting in Table IV the 'pulls' assigned to the colors.

TABLE IV

'PULLS' ASSIGNED THE COLORS

| Color | Boys (647) | Girls (431) |
|---|---|---|
| Yellow | 1.0 | 1.0 |
| Orange | 1.5 | 2.1 |
| Violet | 1.7 | 2.2 |
| Blue | 2.0 | 2.4 |
| Green | 2.0 | 2.8 |
| Red | 2.3 | 2.8 |

Here it will be seen that taking the color Yellow as a base, Red and Green 'pull' equally hard for the girls but not so for the boys, for whom Blue and Green have the same 'pull.' The affective value of Yellow is much less strong for the girls than it is for the boys, for whom it loses its affective value more slowly. It competes

TABLE V

COLOR PREFERENCE SCALES FOR EDUCATIONAL GROUPS

| 1st Grade (113) | | 2nd Grade (111) | | 3rd Grade (142) | | 4th Grade (85) | |
|---|---|---|---|---|---|---|---|
| Color | Scale | Color | Scale | Color | Scale | Color | Scale |
| White | 0 | White | 0 | White | 0 | White | 0 |
| Yellow | 105 | Yellow | 95 | Yellow | 141 | Yellow | 141 |
| Violet | 146 | Orange | 133 | Orange | 260 | Orange | 198 |
| Orange | 153 | Green | 171 | Green | 279 | Violet | 203 |
| Green | 191 | Red | 182 | Violet | 298 | Blue | 225 |
| Blue | 206 | Violet | 208 | Blue | 301 | Green | 232 |
| Red | 217 | Blue | 234 | Red | 320 | Red | 273 |

| 5th Grade (152) | | 6th Grade (144) | | 7th Grade (160) | | High School (171) | | (121) |
|---|---|---|---|---|---|---|---|---|
| Color | Scale | Color | Scale | Color | Scale | Color | Scale | Scale |
| White | 0 | White | 0 | White | 0 | White | 0 | 0 |
| Yellow | 91 | Yellow | 91 | Yellow | 120 | Yellow | 30 | 34 |
| Orange | 163 | Orange | 136 | Violet | 181 | Violet | 60 | 87 |
| Blue | 221 | Violet | 170 | Orange | 188 | Orange | 113 | 132 |
| Green | 236 | Blue | 174 | Blue | 210 | Blue | 143 | 154 |
| Violet | 247 | Green | 204 | Green | 244 | Red | 181 | 195 |
| Red | 300 | Red | 253 | Red | 289 | Green | 200 | 210 |

here sharply with Orange ('pull' 1.5); whereas, with the girls Orange has twice the affective strength of Yellow, the 'pull' increasing more and more until Green and Red have almost three times the affective value of Yellow.

It is not known just what affect the size of the group of judges has on the scale. Here the group for boys is so much the larger that it would be hazardous to speak

in terms of comparative scale values for the sexes. We would not risk such comparisons unless the sizes of the groups were approximately the same.

Table V gives the color preference scales for the educational groups. While the various educational groups here shown are not numerically the same, caution must be used in making direct comparisons. We may say that the educational groups composing the total group of 1078 East Indians do not react to the colors in the same way. In the first place, while White is invariably zero for all educational group scales, Red is not always the most preferred color. This color falls below Violet and Blue in the second grade and below Green in the High School group. Though Yellow is always second to White as it occupies the zero point, Green experiences much fluctuation throughout the grade finally climbing highest in the High School

TABLE VI

COMPARISON OF EAST INDIANS AND AMERICAN WHITES

| | Scale values | | | | | | |
|---|---|---|---|---|---|---|---|
| | white | yellow | orange | violet | blue | green | red |
| East Indians (1078) | 0 | 95 | 160 | 167 | 189 | 204 | 230 |
| Amer. whites (1000) | 0 | 69 | 100 | 107 | 204 | 126 | 122 |
| Diff. | 0 | 26 | 60 | 60 | 15 | 78 | 108 |
| Rank of Diff. | 6 | 4 | 3 | 3 | 5 | 2 | 1 |

group. Orange occupies fourth place in the first grade, drops to third place in the second grade and maintains this position until we reach the seventh grade where it climbs to the third remove from zero. We speak of this because it brings out the evident fact that color preferences change with the changing experiences of these East Indians. In order to make a numerical comparison between the youngest (113) and oldest groups we have derived a scale for a group of the latter of about the same size (121). Note here that for the first grade group Red has a value of 217, but for the older group a value of 195. For the younger group Green has a value of 191 and for the older group a value of 210. In one case, the younger group, Yellow has a scale value of 91 and in the other, the older group, a value of 34. It is easy to believe that some sort of experience has produced the change in affective value of Yellow, Red, and Green and likewise for all the colors.

Such observation warns us against feeling that these color preferences are the result purely of native, racial affective reaction uninfluenced by numerous factors of conditioning. From the data here in hand we note that the scale least influenced by experience must be that of the first grade. This would be true for all racial scales, for here we avoid to some extent the tribal cultural factors. It would be interesting then to compare the scale of the youngest group of East Indians with a similar group of American whites, but before doing so let us compare in Table VI the scale for 1078 East Indians with a scale for 1000 American whites from practically the same educational levels.

Table VI shows that these East Indians agree with American whites in placing White seventh, Yellow sixth, Orange fifth, and Violet fourth. Here the similarity in sequence ceases for the American group places Blue first with a scale value of 204 as most preferred, while the East Indians place Red first with a scale value of 230. The greatest difference in scale value, *i.e.* 108, is found when we compare the re-

spective values assigned to Red. In the case of the color Blue, the scale values are not very different. The difference is 15, and the value for White is the same.

Let us now compare the scales of young East Indians with young American whites (Table VII) and that of older East Indians with that of older American whites (Table VIII). Here it is seen that the scale values of the older students of the two racial groups differ (on the average 54.5) more than those of the younger group (on the average 16.1). In so far as this data goes it seems reasonable to say that in

TABLE VII

COMPARISON OF FIRST GRADE Ss

| Race | Color and scale value | | | | | | |
|---|---|---|---|---|---|---|---|
| | white | yellow | violet | orange | green | blue | red |
| East Indians | 0 | 105 | 146 | 153 | 191 | 206 | 217 |
| American whites | 0 | 120 | 160 | 142 | 149 | 198 | 194 |
| Diff. | 0 | 15 | 14 | 11 | 42 | 8 | 23 |

Av. 16.1

TABLE VIII

COMPARISON OF HIGH SCHOOL Ss

| Race | Color and scale value | | | | | | |
|---|---|---|---|---|---|---|---|
| | white | yellow | violet | orange | blue | red | green |
| East Indians | 0 | 34 | 87 | 132 | 154 | 195 | 210 |
| American whites | 0 | 22 | 78 | 48 | 194 | 48 | 100 |
| Diff. | 0 | 12 | 9 | 84 | 40 | 147 | 110 |

Av. 57.4

color preferences as measured the younger East Indians and American whites are more alike than the older ones. The reasons for this divergence could not be determined, but it seems probable that it is to be found in the nurtural factors.

## SUMMARY

*(1)* The sequence of the color preference of the 1078 East Indians is as follows, beginning with the most preferred: Red, Green, Blue, Violet, Orange, Yellow, White, with much confusion between Orange and Violet.

*(2)* The East Indians have only a slightly longer color preference scale than the American whites.

*(3)* The sexes in the case of the East Indians are fairly similar in their feeling for the colors. The girls feeling no difference for Red and Green; whereas, the boys place Red above Green, but both liked Red.

*(4)* The effects of education and experience seem to produce great changes in the scale values for many of the colors.

*(5)* East Indian color preference scales are rather different from those of American whites.

*(6)* However, the young East Indians and young American whites are more similar than the older East Indians and older American whites. It seems reasonable to suppose that the difference is largely due to nurtural factors.

# COLOR PREFERENCES OF COLLEGE STUDENTS WITH REFERENCE TO CHROMATIC PULL, LEARNING, AND ASSOCIATION

By MARGARET W. ST. GEORGE, University of Denver

The purpose of this study was two-fold: (1) to determine the color preferences of college students; and (2) to determine and to evaluate the factors upon which these preferences depend.

The second part of the problem was studied under the assumptions (a) that color preferences are formed as a result of some affective quality which is native; (b) that they may be formed as a result of experiences in which color is a dominant

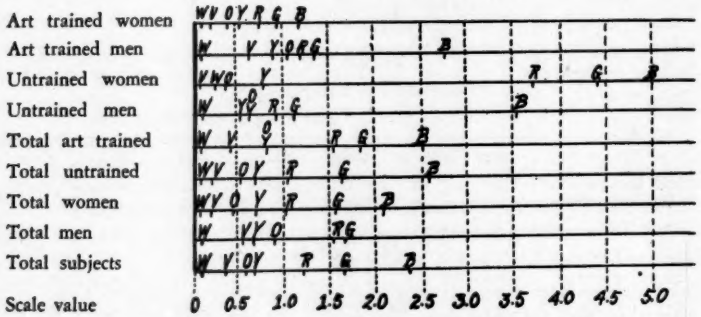


FIG. 1. GRAPHIC REPRESENTATION OF SCALE VALUES OF COLORS

factor; and (c) that they are determined by training received through specialized instruction in art.

*Materials and procedures.* The *S*s were 500 college men and women. They were divided into two groups. One was composed of 250 students having training in color theory and practice. The other group was composed of an equal number having no training of this specific kind.

To determine the color preference of these college students, seven color disks were used. The disks were ½ in. in diam. and were mounted in white cards ¾ in. wide and 1½ in. long. Six of the disks were made of Milton Bradley standard colored papers. A seventh was of white paper suggesting an arbitrary zero value of affectivity.

An envelope containing the seven colors was given every *S* who was told to arrange the disks in order of color preference. After they had arranged the colors

* Accepted for publication February 17, 1938. This study was directed by Professor Thomas R. Garth.

and recorded their preferences, the *Ss* were asked to answer the following questions: (1) Have you ever had any very vivid experience which you associate with one or more of the seven colors presented? If so, describe it fully. (2) Do you have a definite emotional response to any such color dependent upon the incident with which it is associated? (3) Do any of the colors arouse symbolic ideas? For example, does yellow symbolize warmth, exuberance, joy?

*Results.* After the *Ss* had ranked the colors, these rankings were considered under the categories of sex and art-training. In order to find some numerical value for these affective reactions to colors, scales were obtained by the method developed by Thorndike[1] and later used by Hillegas[2] and by Garth.[3] The scales were built upon the arbitrary zero value, white, in terms of D/MD.

Fig. 1 presents graphically color preference scales for the 500 college students, for the 250 art students, for the 250 untrained students, for all the men, and for all the women.

It will be observed that here the college students have a decided preference for blue, green, and red. This is indicated by the extent of the gaps between scale values. There is confusion in the discrimination of feeling for yellow and orange as is evidenced by almost identical scale values. Violet was ranked close to white which was made the arbitrary zero. The greater the distribution over the scale, the better the discrimination of feeling.

TABLE I

FREQUENCY OF KINDS OF COLOR ASSOCIATIONS FOR 500 COLLEGE STUDENTS

| Associations | Frequency | | | Percentage | | |
|---|---|---|---|---|---|---|
| | Art | Non-art | Total | Art | Non-art | Total |
| Nature | 75 | 34 | 109 | 40 | 17 | 28 |
| Objects | 68 | 84 | 152 | 36 | 42 | 39 |
| Locations | 30 | 36 | 66 | 16 | 18 | 17 |
| People | 20 | 61 | 81 | 10 | 30 | 20 |
| Other sensory | 6 | 15 | 21 | 3 | 8 | 5 |
| Imaginative | 6 | 12 | 18 | 3 | 6 | 4 |
| Literature, drama | 4 | 4 | 8 | 2 | 2 | 3 |
| Folklore, superstition | 0 | 4 | 4 | 0 | 2 | 1 |
| Miscellaneous | 9 | 11 | 20 | 5 | 6 | 4 |

Table I presents a frequency distribution of eight types of experience with which color was associated. The table should be read as follows: associations of color with nature were recorded by 75 art students, 40% of that group; by 34 non-art students, 17% of that group; and by 109 of total students or 28%.

Table II is similar to Table I and presents data pertaining to emotional response to color experiences. The table should be read as follows: pleasant emotional responses to color associations were recorded by 104 students, 60% of that group; by 129 non-art students, 64% of that group, and by 233 of total students or 62%.

[1] E. L. Thorndike, *Scale for Measuring the Handwriting of Children*, 1912.
[2] M. B. Hillegas, *A Scale for Measurement of English Composition by Young People*, 1912.
[3] T. R. Garth, *Race Psychology*, 1931, 1-260.

TABLE II

FREQUENCY OF KINDS OF EMOTIONAL RESPONSE TO COLOR FOR 500 COLLEGE STUDENTS

| Emotional Response | Frequency | | | Percentage | | |
|---|---|---|---|---|---|---|
| | Art | Non-art | Total | Art | Non-art | Total |
| Pleasant | 104 | 129 | 233 | 60 | 64 | 62 |
| Unpleasant | 39 | 56 | 95 | 26 | 30 | 26 |

Table III presents data pertaining to color symbolism. This table should be read as follows: abstract color symbolism was recorded by 128 art students, 86% of that group; by 180 non-art students, 78% of that group; and by 308 of total students or 82%.

TABLE III

FREQUENCY OF COLOR SYMBOLISM FOR 500 COLLEGE STUDENTS

| Color Symbolism | Frequency | | | Percentage | | |
|---|---|---|---|---|---|---|
| | Art | Non-art | Total | Art | Non-art | Total |
| Abstract | 128 | 180 | 308 | 86 | 78 | 82 |
| Concrete | 23 | 48 | 71 | 15 | 21 | 19 |

## SUMMARY

*(1)* The order of color preference for college students was blue, green, red, yellow, orange, violet, white.

*(2)* Color preferences depended partly upon associations of color with affections.

*(3)* Certain colors, because of a native 'pull' were most commonly preferred, *i.e.* cool, recessive hues were more favored than warm, aggressive ones.

*(4)* Scales of color preference for art students and non-art students were so similar that it may be inferred that art education did not function actively. The sequence of preference for art students was blue, green, red, orange, yellow, violet, white. The sequence of preference for non-art students was blue, green, red, yellow, orange, violet, white.

*(5)* There was no significant difference in the sequences attributable to sex. The sequence for men was blue, green, red, orange, yellow, violet, white. The sequence for women was blue, green, red, yellow, orange, violet, white.

*(6)* Associations with colors were a general trend. They were recorded more frequently by non-art students than by art students.

*(7)* Emotional response usually accompanied color experience. Responses were recorded more frequently by non-art students than by art students.

*(8)* Color symbolism seemed uncommon, being more disposed to be common among non-art students than art students.

## MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

### LXXXVII. The Effect of Light-Flashes During the Course of Dark Adaptation

By E. A. Suchman and H. P. Weld

In a recent study Allen and Dallenbach found that light-flashes of constant intensity and duration introduced at different intervals in the course of dark adaptation caused a temporary rise and then a fall in the adaptation curve, but otherwise did not alter it.[1] This result, as the authors indicate, is not in accord with a statement made by Aubert and confirmed by Nagel, namely, that the effect of the flashes is to lower the threshold. Allen and Dallenbach suggest, however, that the matter cannot be considered as definitely settled until both the intensity and the duration of the light-flashes are also varied. The purpose of our study was to investigate the effect of these two conditions.

#### Method and Procedure

The apparatus and the conduct of the experiment was in all but two minor changes exactly the same as in the Allen and Dallenbach experiment, and consequently need not be described here. The minor changes, which were merely precautionary, consisted first in placing gray screens on both sides of *O* and on the ceiling and floor to insure a constant neutral background and to prevent stray light from entering the eye. Secondly, to make sure of a constant voltage for the light-flashes, the current producing the flash was introduced into the adaptometer circuit containing the rheostat and volt-meter. Since *O* did not make any adjustments during the period of the flash, this change did not interfere with the measurements.

The flashes were produced by a flood-lamp over which a filter-box was fitted. The following combinations of filters were used to produce the desired intensities as measured by a General Electric photometer: (1) 15 foot candles, a 75-w. white frosted bulb, no filters; (2) 1 foot candle, a 100-w. daylight bulb, 2 frosted glass filters; and, (3) threshold value, a 60-w. daylight bulb, 4 frosted glass filters, a cardboard filter containing ¼ in. holes. The 'threshold value' was slightly higher than the strict threshold and was obtained by changing the number of holes in the cardboard filter until *O* could just perceive the flash after 10 min. of dark adaptation.

The duration of the flashes of constant intensity was controlled by the condenser-charge timer, and were 0.25 sec., 1 sec., 10 sec., and 30 sec. The duration of the flashes with varying intensities was 0.25 sec.

*Instructions.* At the beginning of the experiment, *O* was given the following instructions:

* Accepted for publication December 1, 1937.

[1] L. K. Allen and K. M. Dallenbach, The effect of light-flashes during the course of dark-adaptation, this Journal, 51, 1938, 540-548.

With your teeth in the biting board, you will fixate the black dot on the screen. At the 'ready-now' signal, the lights will go out. You will then fixate a small red dot which will appear on the screen directly in front of you, and turn the handle slowly to the left until a light below the fixation-point is just noticeable. You will be permitted to make small adjustments, if necessary. As soon as you are satisfied that the light can just be seen, report 'there' and immediately close your eye and release the handle until the next signal. A light may be flashed upon the screen during the course of the experiment. Do not close your eye or make any adjustments until the light goes off. Report any after-effects when you have completed your adjustments.

*Observers.* Three *O*s were used throughout the entire experiment.[2] All were trained in psychological observation, *Su* and *We,* were graduate students in psychology, and *Gl* was an undergraduate major in psychology. *Su* and *We* were already practiced and experienced in dark-adaptation, having served as *O*s for Allen and Dallenbach. *Su* and *We* possessed normal vision, while *Gl* was required to wear glasses during experimentation. *We* was partially colorblind. *Su* and *Gl* had brown eyes, *We* blue eyes. Only *Su* knew the purpose of the experiment.

*Procedure.* The method, a modification of the psychophysical method of limits, was the same as that employed by Allen and Dallenbach. Only ascending continuous series were used. Descending series were omitted because supraliminal stimuli would interfere with the course of the dark-adaptation and would introduce an error of 'form-perception' in place of minimal-light perception. In view of the fact, determined by Helson and Fehrer,[3] that the limen for form-perception is 12 to 25 times higher than that for the light minimum, the *O*s were warned to report the moment the light-stimulus appeared, and not to wait until the triangle could be seen.

To avoid errors of anticipation, the initial settings of the adaptometer were varied so as to give an equal number of long, short and medium series. *O* made his adjustments until the minimum light could just be seen, reported "there" and closed his eye. *E* recorded the scale reading, lowered the intensity of illumination below *O*'s threshold, checked the voltage and time, and gave the "ready, now" signal for the next observation. This procedure was repeated at a fairly steady rate every 30 sec. for 40 min. The flash was given after 25 min. of dark-adaptation. At this time the curve of dark adaptation was fairly well levelled, and the retina was assumed to be in a sufficiently stable state. The flashes were introduced immediately following a "Ready, now" signal and *O* made his adjustments and reported any after-effects as soon as the flash disappeared.

The following flashes were given an equal number of times in haphazard order, (1) Threshold intensity for 0.25 sec., (2) 1 foot candle for 0.25 sec., (3) 15 foot candles for 0.25 sec., (4) 1 foot candle for 1 sec., (5) 1 foot candle for 10 sec., (6) 1 foot candle for 30 sec. These values were chosen for detailed study, after an extended period of preliminary experimentation, as most clearly illustrating the effect of flashes of varying intensity and duration upon the course of dark-adaptation. Every experimental condition was repeated five times for each *O*, and only one during an experimental period. A series of five normal observations, during which no flash was given was interspersed among the above periods.

[2] The *O*s were Mr. E. A. Suchman (*Su*), Mr. Henry Werner (*We*), and Mr. I. Glassgold (*Gl*). Miss L. K. Allen served as experimenter when *Su* was observer.

[3] H. Helson and E. V. Fehrer, The rôle of form in perception, this JOURNAL, 44, 1932, 79-102.

Practice periods were necessary only in the case of *Gl*, who had never worked with dark-adaptation before. These were continued until the curve for the normal course of dark-adaptation had become relatively stable and regular.

## RESULTS

*Normal course of dark adaptation.* The normal course of dark adaptation was determined for every *O* to serve as a control for the course of adaptation with the introduction of flashes. The stimulus thresholds are shown in Table I. Values are

TABLE I

THRESHOLDS DURING THE ENTIRE COURSE OF A TYPICAL NORMAL DARK-ADAPTATION PERIOD, SHOWING ALL SETTINGS

| O | 5-min. periods | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| | 880,000 | 3,000 | 700 | 400 | 450 | 300 | 300 | 200 |
| | 240,000 | 2,200 | 600 | 300 | 450 | 250 | 300 | 400 |
| | 72,000 | 3,400 | 500 | 500 | 300 | 350 | 200 | 300 |
| | | 3,200 | 300 | 300 | 400 | 200 | 160 | 300 |
| | 52,000 | 2,800 | 400 | 400 | 400 | 250 | 300 | 300 |
| We | 20,000 | 1,200 | 350 | 400 | 400 | 140 | 350 | 300 |
| | 12,000 | 1,000 | 300 | 400 | 200 | 300 | 300 | 300 |
| | 2,200 | 800 | 350 | 450 | 250 | 200 | 200 | 250 |
| | 1,400 | 700 | 300 | 180 | 300 | 180 | 250 | 400 |
| | | 800 | 400 | 400 | 250 | 200 | | 300 |
| | | | | | | 200 | | 200 |
| | 3,200,000 | 16,000 | 3,800 | 1,600 | 600 | 400 | 200 | 200 |
| | 400,000 | 12,000 | 2,600 | 1,200 | 400 | 400 | 400 | 400 |
| | 160,000 | 10,000 | 2,000 | 1,200 | 400 | 400 | 400 | 400 |
| | 170,000 | 7,500 | 2,200 | 1,200 | 400 | 400 | 400 | 200 |
| Su | 88,000 | 10,000 | 2,200 | 800 | 400 | 400 | 400 | 400 |
| | 56,000 | 5,200 | 1,800 | 600 | 600 | 400 | 600 | 200 |
| | 40,000 | 4,600 | 1,400 | 800 | 400 | 400 | 400 | 400 |
| | 24,000 | 4,000 | 1,000 | 800 | 400 | 200 | 600 | 400 |
| | 28,000 | 4,200 | 1,000 | 800 | 400 | 600 | 400 | 400 |
| | 20,000 | 4,600 | 1,600 | 1,000 | | 400 | | 400 |
| | | | | 600 | | 400 | | |
| | 2,800,000 | 36,000 | 3,000 | 2,000 | 1,300 | 950 | 1,000 | 1,200 |
| | 800,000 | 28,000 | 5,200 | 1,600 | 1,100 | 850 | 600 | 800 |
| | 240,000 | 16,000 | 3,400 | 1,500 | 1,300 | 800 | 1,400 | 800 |
| Gl | 120,000 | 5,400 | 2,800 | 1,200 | 1,100 | 600 | 800 | 850 |
| | 72,000 | 4,400 | 2,600 | 1,400 | 1,200 | 900 | 1,000 | 1,000 |
| | 60,000 | 4,200 | 2,200 | 1,200 | 800 | 800 | 2,000 | 800 |
| | 56,000 | 3,400 | 2,000 | 1,300 | 900 | 1,000 | 800 | 1,000 |
| | | | | | | 750 | | 800 |
| | | | | | | 1,100 | | |
| | | | | | | 800 | | |
| | | | | | | 1,200 | | |

given for each *O* for every 5 min. period of each experiment. In addition to the determination of this normal course, it will be recalled that a normal series (without flash) was interspersed among the experiments in which the flash was introduced. These results are given as series *N* in Tables II and III. In Table II the values are averages of 5 observations for each *O* for every 5-min. period up to the 25th min.

and from the 30th to the 40th min. In Table III the figures represent detailed averages of the five determinations of the 25-30 min. period divided into 10 equal parts according to Vincent's method.

Our results agree so closely with those of previous investigators that we have not plotted the normal curve, and in Figs. 1 and 2, we have omitted the normal

TABLE II

AVERAGE THRESHOLDS FOR EVERY O FOR ALL BUT THE SIXTH 5-MIN. INTERVAL

| O | Series | 5-min. periods | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | 1 | 2 | 3 | 4 | 5 | 7 | 8 |
| *We* | N | 182,714 | 17,734 | 340 | 208 | 230 | 287 | 312 |
| | T—.25 | 203,219 | 29,128 | 760 | 407 | 287 | 275 | 268 |
| | Int. 1—.25 | 188,280 | 21,750 | 760 | 459 | 377 | 320 | 258 |
| | 15—.25 | 135,651 | 22,243 | 626 | 328 | 340 | 256 | 248 |
| | 1— 1 | 156,373 | 12,054 | 541 | 372 | 285 | 349 | 227 |
| | Dur. 1—10 | 195,517 | 20,157 | 523 | 370 | 288 | 197 | 209 |
| | 1—30 | 205,233 | 17,076 | 487 | 324 | 283 | 234 | 232 |
| *Su* | N | 631,863 | 36,229 | 1,995 | 950 | 750 | 575 | 570 |
| | T—.25 | 519,256 | 42,360 | 1,522 | 1,056 | 880 | 722 | 659 |
| | Int. 1—.25 | 530,000 | 12,485 | 1,869 | 975 | 825 | 710 | 680 |
| | 15—.25 | 615,300 | 33,670 | 1,580 | 728 | 618 | 539 | 663 |
| | 1— 1 | 604,221 | 26,721 | 1,720 | 946 | 826 | 610 | 610 |
| | Dur. 1—10 | 639,098 | 108,240 | 1,562 | 862 | 621 | 527 | 520 |
| | 1—30 | 513,673 | 81,660 | 1,407 | 847 | 669 | 680 | 645 |
| *Gl* | N | 451,800 | 79,800 | 1,868 | 1,155 | 965 | 660 | 748 |
| | T—.25 | 466,904 | 112,272 | 2,515 | 1,406 | 1,006 | 877 | 877 |
| | Int. 1—.25 | 378,417 | 42,818 | 1,422 | 998 | 794 | 826 | 659 |
| | 15—.25 | 512,711 | 178,513 | 2,800 | 1,446 | 1,161 | 1,208 | 1,286 |
| | 1— 1 | 505,545 | 175,236 | 3,284 | 1,725 | 1,471 | 1,374 | 1,109 |
| | Dur. 1—10 | 350,380 | 78,485 | 1,607 | 1,110 | 855 | 928 | 765 |
| | 1—30 | 431,263 | 85,764 | 2,389 | 1,556 | 1,136 | 1,019 | 1,056 |

curves for the first 15 min. of the 'flash-series.' The coefficients in Tables I and II show the familiar rapid drop and thereafter slow decrease in the threshold. There are however individual differences in the rate and final level of dark adaptation obtained. *We* reaches his final level of dark adaptation after 10 min., while *Su* and Gl take approximately 15 min. Furthermore, *We*'s final limit is between 200 and 300 units, which is lower than those for both *Su* and *Gl* whose final limits are 600-700, and 800-1,000 respectively. Both *Su* and *Gl* also show more variability in their curves than does *We*.

There is also a decided practice effect. Both *Su* and *We*, revealed a practice effect although they had served as observers in Allen and Dallenbach's experiment. In that experiment *We*'s final limit was about 1,250 units and *Su*'s was about 2,500. At the conclusion of our investigation *We*'s limit had fallen to about 300 and *Su*'s to about 600 units. At the beginning of our experiments *Gl* had practiced until he had reached a limit of about 2,000 but at the end of it had fallen further to about 900. The practice effect appears to have reached its final limit with *Su* and *We* who remained at the same level for the entire latter half of our experiment.

*Effect of flashes of different duration.* In Table II are shown the average stimulus thresholds for every *O* for all but the 25-30 min. interval when the intensity was constant at 1 ft.c. for the 4 durations of 0.25, 1, 10, and 30 sec. In Table III are the thresholds interpolated by the Vincent method during the 25-30 min. interval when the flash occurred. The values found after the first 15 min. of the course of adaptation are plotted in Fig. 1.

It will at once be seen that the general effect of the flashes of all 4 durations was a sudden rise and immediately thereafter a gradual decline in the threshold. The

TABLE III

THRESHOLDS FOR EVERY O DURING THE SIXTH 5-MIN. PERIOD

| O | Series | Av. 25-min. setting | Values interpolated by the Vincent method | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
| We | N | 300 | 290 | 306 | 308 | 280 | 248 | 254 | 307 | 325 | 252 | 262 | 325 |
| | T—.25 | 230 | 310 | 260 | 210 | 250 | 244 | 260 | 226 | 202 | 284 | 290 | 206 |
| | Int. 1—.25 | 354 | 480 | 312 | 358 | 318 | 320 | 292 | 340 | 282 | 260 | 290 | 236 |
| | 15—.25 | 340 | 670 | 330 | 280 | 226 | 292 | 290 | 330 | 272 | 282 | 290 | 260 |
| | 1— 1 | 280 | 740 | 274 | 326 | 278 | 234 | 228 | 236 | 292 | 198 | 268 | 236 |
| | Dur. 1—10 | 272 | 1,820 | 700 | 410 | 330 | 264 | 256 | 210 | 246 | 276 | 210 | 198 |
| | 1—30 | 224 | 2,320 | 940 | 760 | 590 | 420 | 340 | 350 | 264 | 232 | 248 | 226 |
| Su | N | 680 | 680 | 610 | 560 | 580 | 570 | 640 | 640 | 630 | 700 | 700 | 700 |
| | T—.25 | 800 | 760 | 700 | 710 | 800 | 710 | 720 | 670 | 660 | 630 | 660 | 760 |
| | Int. 1—.25 | 720 | 1,040 | 830 | 670 | 695 | 780 | 760 | 840 | 860 | 790 | 690 | 730 |
| | 15—.25 | 600 | 4,080 | 1,260 | 770 | 690 | 600 | 480 | 540 | 600 | 580 | 590 | 6,600 |
| | 1— 1 | 760 | 1,760 | 790 | 670 | 730 | 760 | 700 | 780 | 810 | 770 | 700 | 640 |
| | Dur. 1—10 | 560 | 5,680 | 2,820 | 1,560 | 1,000 | 800 | 780 | 720 | 580 | 580 | 520 | 560 |
| | 1—30 | 680 | 12,700 | 7,640 | 3,200 | 2,200 | 1,460 | 960 | 860 | 760 | 750 | 700 | 840 |
| Gl | N | 900 | 1,090 | 810 | 800 | 800 | 860 | 820 | 820 | 730 | 780 | 770 | 810 |
| | T—.25 | 980 | 1,040 | 1,160 | 1,000 | 1,120 | 1,000 | 1,010 | 1,120 | 930 | 1,030 | 1,020 | 1,100 |
| | Int. 1—.25 | 700 | 1,720 | 860 | 800 | 780 | 800 | 780 | 940 | 1,000 | 870 | 870 | 920 |
| | 15—.25 | 1,100 | 4,160 | 1,100 | 1,000 | 1,060 | 1,020 | 1,020 | 1,160 | 1,050 | 940 | 940 | 880 |
| | 1— 1 | 920 | 4,640 | 1,680 | 1,280 | 1,140 | 1,280 | 1,160 | 1,320 | 1,120 | 1,120 | 980 | 850 |
| | Dur. 1—10 | 860 | 7,360 | 2,000 | 1,520 | 1,360 | 1,200 | 1,000 | 920 | 1,040 | 1,280 | 920 | 960 |
| | 1—30 | 920 | 14,000 | 4,720 | 2,720 | 2,120 | 1,800 | 1,640 | 1,400 | 1,020 | 1,120 | 980 | 900 |

longer the duration, however, the greater the rise in the threshold, and the longer the time required to return to normal. This is true for all *Os*. The entire effect never lasts for more than 5 min. and there is no ultimate effect whatever.

*Effect of flashes of different intensities.* An examination of Tables II and III and of Fig. 2 will show the effect of flashes of different intensities but of constant duration. The only effect is that of a temporary rise in the threshold. The amount of rise in the threshold varies, however, with the intensity of the flash; the higher the intensity the greater the rise in the threshold. Indeed the effect of the 'threshold' intensity is so slight as to be negligible. The duration of this effect appears to be independent of the intensities employed in this experiment. The return to the former level occurred almost immediately. Here also there is no effect upon the final degree of dark adaptation.

*Discussion.* Our results support Allen and Dallenbach's contradiction of the statement of Aubert that "the progress of dark adaptation was not only not impeded by flashes of light, but that, in fact, it was aided in some measure," and also the statement of Nagel to the effect that Aubert's observation "had since been confirmed many times. . . . That the instantaneous light of a match reflected from dark walls into the observer's eye lowers the threshold of the light considerably, as much perhaps as ⅓ can easily be verified." Our results also agree with Parson's con-

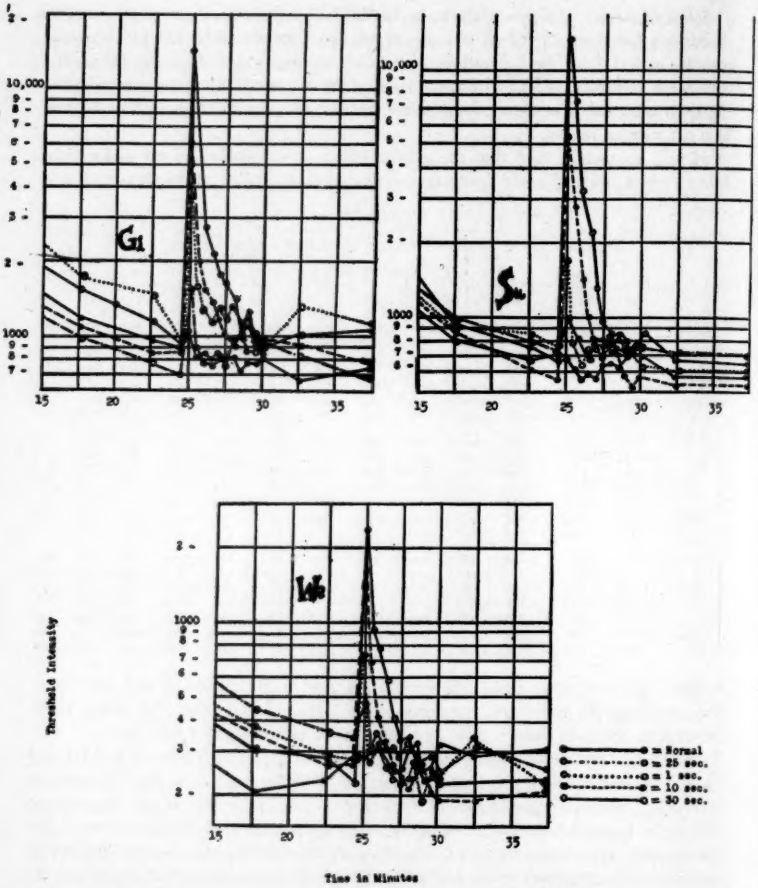


FIG. 1. DARK ADAPTATION CURVES FROM THE 15TH TO THE 40TH MINUTE OF THEIR COURSE SHOWING THE EFFECT OF FLASHES OF VARYING DURATION

(Intensity of the flash constant at 1 footcandle.)

clusion that exposure to bright light does not materially alter the course of the curve; but they contradict his statement that this exposure causes a temporary fall in the curve.

A detailed study of Figs. 1 and 2 shows that there is a definite relationship between the intensity and duration of the flash, used interchangeably, and the amount of decrease in sensitivity. This relationship, so far as the gross measure-

ments of this experiment go, agrees with the Bunsen-Roscoe law of $It = C$, advanced by Hecht and others for the photoreceptor process, where $I$ equals intensity, $t$ equals time, and $C$ is a constant.[4] This would lead us to expect that the same rise in the threshold would be effected by a stimulus of $x$ intensity for a duration of $y$, a stimulus of $\frac{1}{2}x$ for a duration of $2y$, and a stimulus for a $2x$ for a duration of $\frac{1}{2}y$. Our results roughly approximate this rule. Caution must be taken, however, to limit this reciptical relationship to the immediate rise of threshold-value only, and not for the duration or rate of fall of the after-effect, which, as can be seen from the graphs and from Table IV, is dependent entirely upon the duration and not the intensity of the flash. No exact measurements were made of the duration of this after-effect, but since settings were made every 30 sec., it may be approximated as in

TABLE IV

DURATION AND RELATIVE INCREASE OF THESHOLD IMMEDIATELY FOLLOWING THE FLASH FOR EVERY O

| O | Series | Rel. Inc. | Duration (sec.) |
|---|---|---|---|
| *We* | T—.25 | none | — |
| | Int. 1—.25 | 1.5 | 5 |
| | 15—.25 | 2.5 | 10 |
| | 1— 1 | 2.5 | 15 |
| | Dur. 1—10 | 6.5 | 60 |
| | 1—30 | 9.0 | 120 |
| *Su* | T—.25 | none | — |
| | Int. 1—.25 | 1.5 | 5 |
| | 15—.25 | 6.5 | 30 |
| | 1— 1 | 2.5 | 30 |
| | Dur. 1·—10 | 9.5 | 120 |
| | 1—30 | 17.0 | 210 |
| *Gl* | T—.25 | none | — |
| | Int. 1—.25 | 2.0 | 5 |
| | 15—.25 | 4.0 | 15 |
| | 1— 1 | 4.0 | 30 |
| | Dur. 1—10 | 8.5 | 90 |
| | 1—30 | 14.5 | 180 |

Table IV. The time and rate of fall of the threshold is seen to vary directly with the duration of the previous light-excitation period. This is true for all *O*s.

Analysis of the relative increase in threshold intensity following flashes of varying duration as shown in Fig. 3 reveals another significant relationship. From these curves it will be seen that the relative increase in the threshold decreases as the duration of the flash increases. For example, the relative increase in threshold following a flash of 10 sec. is 2.5 (*We*), 2.5 (*Gl*), and 3.5 (*Su*) times greater than that of a flash of 1 sec., while a flash of 30 sec. is only 1.5 (*We*), 1.5 (*Gl*), and 2.0 (*Su*) times greater than a flash of 10 sec. This is graphically shown by the definite leveling off of the upper portion of the curves in Fig. 3.

Our results also reveal a possible relation between the duration of the rise in

[4] S. Hecht, The nature of photoreceptor process, in *The Foundations of Experimental Psychology*, 1929, 218 ff.

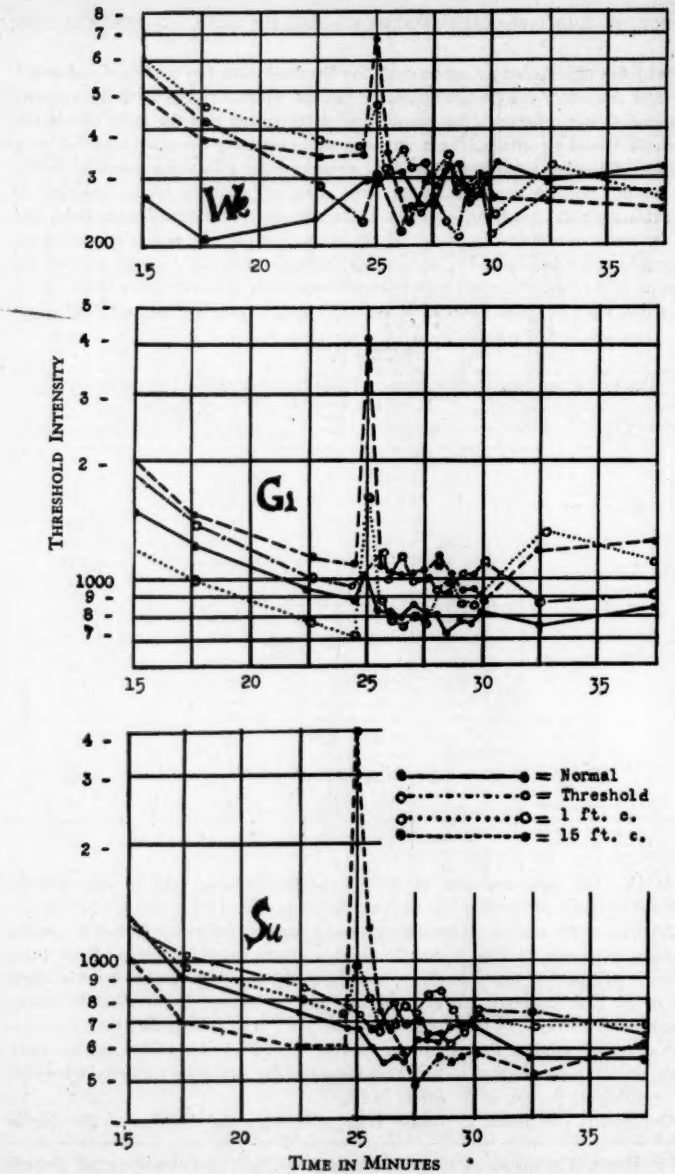


FIG. 2. ADAPTATION CURVES FROM THE 15TH TO THE 40TH MINUTE SHOWING THE EFFECT OF FLASHES OF VARYING INTENSITY

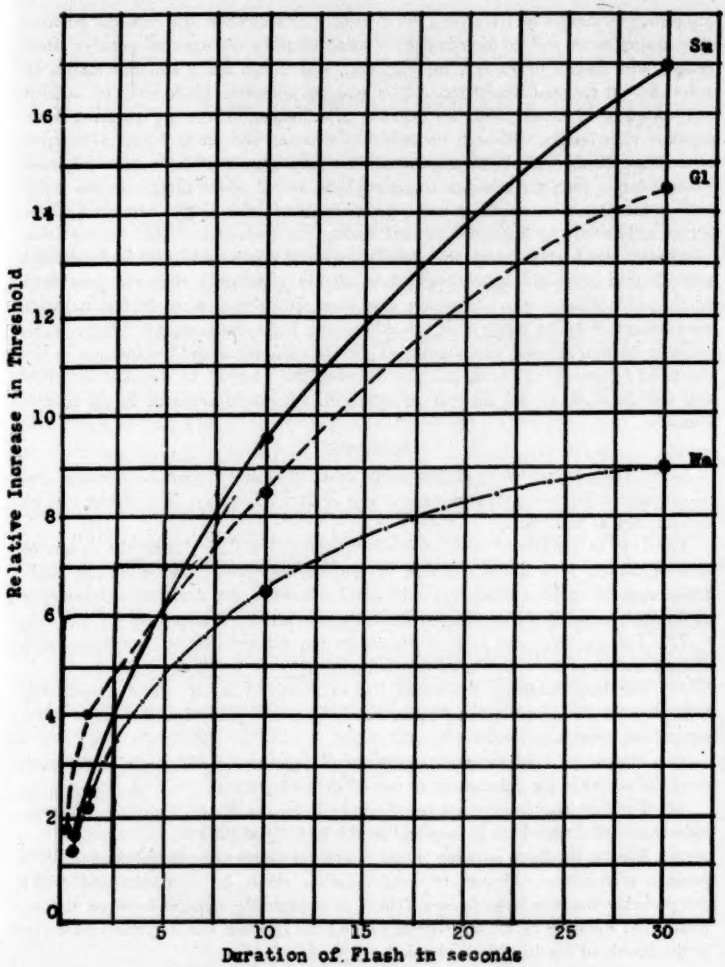


FIG. 3. RELATIVE INCREASE OF THRESHOLD FOLLOWING FLASHES OF VARYING DURATION FOR ALL *Os*

the threshold and the presence of after-images. There was apparently a constant relationship between the type of after-image and the duration and intensity of the preceding flash. No after-images were reported by any of the *Os* for a flash of threshold-value and of duration of 0.25 sec. *Su* and *Gl* reported positive after-

images for all flashes of 1 ft.c. at a duration of 0.25 sec. while *We* reported positive after-images twice and no after-image 3 times. All three *Os* reported positive after-images with flashes of 15 ft.c. for 0.25 sec., and 1 ft.c. for 1 sec. For flashes of 1 ft.c. for 10 sec. and for 30 sec. all *Os* reported complete 'black-outs,' *i.e.* sudden and complete blackness with no positive after-images. These we presume were negative after-images, although on only one occasion did an *O* report a negative after-image. In this case the *O* (*Su*) described the image "as a large white-bordered triangle larger than the stimulus triangle which moved about slightly in the lower half of the projection field." It seems probable that in this single instance *Su* happened accidentally to fixate the triangle during the flash-period. Had his eye been directed to the fixation point, the after-image could not have had a clearly defined contour, and it would have appeared as darker ('blacker') than the gray field of the dark adapted eye. Assuming that these 'black-outs' were in fact negative after-images it seems likely that, as Allen and Dallenbach suggest,[8] there is an intimate relation between the intensity of the after-sensations and the increase in the threshold following the flash, and also between the duration of the after-sensation and the duration of the interval in which the threshold returned to its normal course.

## Summary

In this study we sought to determine the effect of flashes of various intensities and of various durations upon the normal course of dark adaptation. The results may be summarized as follows.

*(1)* Loss of sensitivity as a result of exposure to a flash during the course of dark-adaptation is a direct function of the intensity and duration of the flash. There appears to be a reciprocal relationship between the duration and intensity of the flash as far as the momentary decrease in sensitivity is concerned.

*(2)* The duration and rate of return to the former state of sensitivity is a direct function of the duration of the flash alone.

*(3)* For long durations the initial rise in threshold is very rapid immediately following the flash, and the decline in threshold occurs gradually. The entire effect never lasts more than 5 min.

*(4)* Flashes of light for all intensities and durations employed in this experiment do not alter the subsequent course of dark-adaptation.

*(5)* The dark-adaptation curve has the same shape for all *Os*. There are, however, individual differences both in rate and in the final limit attained.

*(6)* Flashes of short duration at all intensities above the threshold resulted in positive after-images. Flashes of long duration result in after-sensations which are probably negative after-images. There is a probable relation between the intensity and duration of the after-sensation and the increase and duration of the rise in the threshold resulting from the flash.

[8] *Op. cit.*, 547.

## APPARATUS

# A MECHANICAL CORRELATOR

By C. M. HARSH and S. S. STEVENS, Harvard University

Galton's estimation of the correlation coefficient from the angles of the regression lines was mathematically crude, and since that time the trend has been toward refinements of the method of least squares which, although more arduous, is certainly more elegant. On the other hand, the mathematical accuracy of the product-moment coefficient is considerably greater than is necessary in many present-day psychological investigations, especially those of a preliminary nature. Accurate commercial machines for obtaining coefficients of correlation are either very expensive and delicate, or their use involves nearly as much work as the straightforward determination of the coefficients with the aid of ordinary calculating machines. There is needed a device, simple and rapid in its operation, which will yield directly a reasonable approximation to the true value of the product-moment coefficient of correlation.

We have constructed a mechanical correlator into which the raw data are entered in the form of steel balls and which, after a few mechanical operations, yields the necessary regression coefficients from which the coefficient of correlation is obtained by a simple operation on a slide-rule. This correlator is useful in the survey of large numbers of functional relations where economy of time is more important than absolute accuracy. It is also particularly valuable for classroom demonstration of the theory of correlation.

Each regression coefficient is, of course, the tangent of the angle between an axis and the line of regression on that axis. From the familiar formulas for the slopes of the regression lines

$$b_{xy} = r_{xy} \cdot \sigma_x/\sigma_y, \qquad b_{yx} = r_{yx} \cdot \sigma_y/\sigma_x,$$

it follows that

$$b_{xy} \cdot b_{yx} \equiv \tan \theta_{yx} \cdot \tan \theta_{xy} = (r_{xy})^2,$$

or

$$r_{xy} = [\tan \theta_{yx} \cdot \tan \theta_{xy}]^{1/2}$$

where $\theta_{yx}$ is the angle between the $x$-axis and the line of regression of $x$ on $y$. The principle of the correlator is to find the means of the arrays by mechanical balancing and then visually to estimate the regression lines which best fit the means. If the regression lines are rectilinear and if their slopes can be closely approximated, the correlation coefficient is easily found without calculating standard deviations.

The correlator, pictured in Figs. 1 and 2, consists of a square of 196 tubular cells in which we plot the scatter diagram directly from the data by letting individual items be represented by uniform 5/16 in. steel balls dropped in the appropriate cells. The steel balls are held in line by the walls of the cells, but their weight rests upon the balancing table directly below. Milled brass collars on the tubular cells allow the cells to be firmly supported by either of two rigid steel combs, each consisting of fifteen bars of $\frac{3}{16}$ in. by $\frac{1}{4}$ in. milled steel, held parallel by a heavy clamp on one end. These combs fit into metal guides on the supporting frame of the correlator. Thus they are always accurately centered over the balancing table, although quickly removed or inserted. The top surface of one comb is $\frac{1}{8}$ in. below the bottom surface

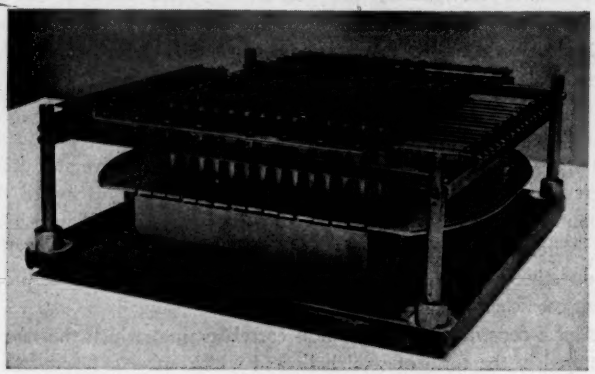

FIG. 1. CORRELATOR: GENERAL VIEW WITH BOTH STEEL COMBS INSERTED

The tubular cells are ready to receive the steel balls which represent the raw data to be correlated. Below the tubular cells can be seen the ends of the balances on which the means of the arrays are determined.

of the other, to allow for the thickness of the middle supporting collars of the tubular cells. With both combs in place the cells are held stationary so that the data can be 'plotted.' When the $x$-comb (parallel to the $x$-axis) is removed, the $x$-arrays of cells may be slid back and forth for balancing to find the mean of each array. Similarly when the $x$-comb is inserted and the $y$-comb removed, the $y$-arrays may be balanced.

The balancing table is composed of fourteen aluminum strips, centered under the cell arrays and delicately balanced on a shaft to facilitate rapid determination of the means of the arrays. Permanent stops under the ends of the strips limit their vertical movement to less than $\frac{1}{8}$ in., and thereby prevent the balls from escaping from the tubular cells. By raising two movable stops on a lower-level aluminum table the fourteen strips are locked together, and the entire correlation surface can then be balanced on a knife edge, for finding the mean of the total distribution, $M_{xy}$. The entire balancing mechanism is mounted on a steel platform which revolves on a ballbearing track. A spring lock holds it in line with the $x$-arrays or, turned through 90°, in line with the $y$-arrays.

In operating the correlator, abscissa and ordinate scales are prepared which fit the data to a maximum of 14 class-intervals. After the correlation surface is plotted from the raw data, the $x$-comb is removed, the balancing strips are locked together by the movable stops, and with the aid of a square steel frame the entire block of cells is slid horizontally until the correlation surface balances. The mean, $M_y$, is

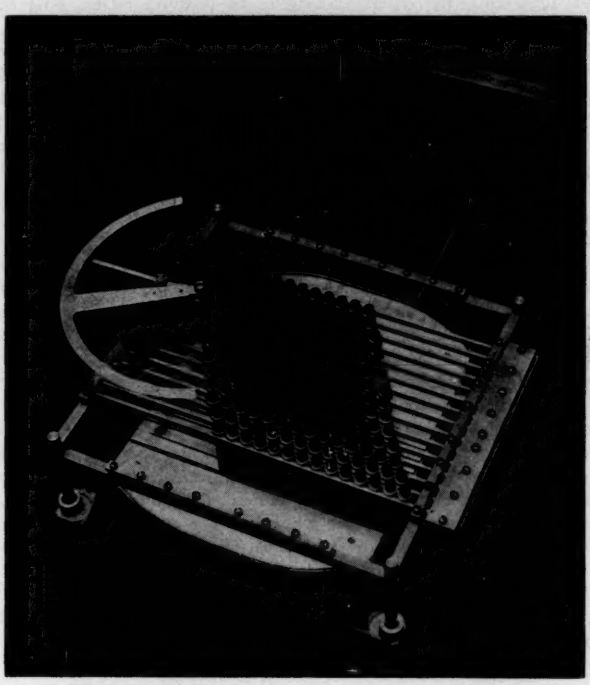

FIG. 2. CORRELATOR WITH THE $X$-ARRAYS INDIVIDUALLY BALANCED

The edge of the protractor has been fitted along the regression line formed by the cells. From the protractor the tangent of the regression line is read directly.

read off of a scale, the cells are returned to center, the $x$-comb inserted, the $y$-comb removed, and the balancing platform is revolved 90°. (The balancing table is wide enough to prevent balls from falling out of the cells during rotation.) The mean, $M_x$, is similarly determined, and then the movable stops are lowered to allow balancing of the separate $y$-arrays. When the $y$-arrays are balanced, the means of the arrays are in line over the fulcrum and the end-cells of the arrays are out of line (see Fig. 2), and form an angle which is the negative of the angle between the $y$-axis and the line of regression of $x$ on $y$.

The regression line is fitted with the aid of the semicircular tangent scale which

is easily clamped onto either comb. Sliding metal coördinate scales allow the center of the regression line to be adjusted to the midpoint ($M_x$, $M_y$). A black line on a transparent celluloid surface pivots about this point ($M_x$, $M_y$) and is aligned by eye with the end-cells of the arrays. A pointer on the semicircular tangent scale gives the value of tan $\theta_{xy}$ directly. (Fig. 2 shows this stage of the operation.) The cells are then re-centered, the combs exchanged, the platform rotated 90°, and the $x$-arrays balanced to determine the line of regression of $y$ on $x$ and tan $\theta_{yx}$.

With the aid of a slide-rule the correlation coefficient is quickly computed by taking the square root of the product of the two tangents. With a little practice the entire procedure, aside from the plotting of the data, can be completed in 10 minutes. The plotting with the steel balls is done as rapidly as plotting on coördinate paper, so the total saving of time is considerable. To clear the machine for the next plot the supporting frame is tilted up, allowing the balls to roll out of the cells and into a trough.

As is to be expected, the correlator is not extremely accurate, owing to the visual method of estimating the regression lines, although with practice, an operator can greatly increase his skill in determining the best fitting lines. Part of the skill of alignment is in weighting the arrays according to their sub-populations, and according to their distances from the mean. Even when this is done quickly, the tangents can be estimated with surprising accuracy. If the arrays are extremely irregular, the fitting of the regression line becomes more difficult, as it does when the regression is curvilinear. In the latter case, however, one can see at a glance that the regression is curvilinear and that $\eta$'s should be calculated.

In carrying out a number of correlations of all sizes it has been found that the usual limits of accuracy are from 0.02 to 0.04 in the coefficient. The accuracy appears to be greatest in the range from 0.6 to 0.9, and less in the ranges of very low and very high correlations, probably because of the nature of the tangent scales, and of the constant error of visual estimation. For curvilinear regression lines ($\eta$ around 0.6) the error is usually within 0.05. These determinations refer to populations between 80 and 200 individuals. With smaller populations the accuracy might well become lower, although this possibility has not been verified. On the whole, however, the correlator gives results good enough to indicate what relations are worth investigating in more detail.

# A TIMING RELAY FOR PSYCHOPHYSICAL EXPERIMENTS

By JOHN VOLKMANN, Columbia University, and RALPH GERBRANDS
Harvard University

The temporal variables in psychophysical experiments are often important ones, and it is desirable to control them by some relatively simple,

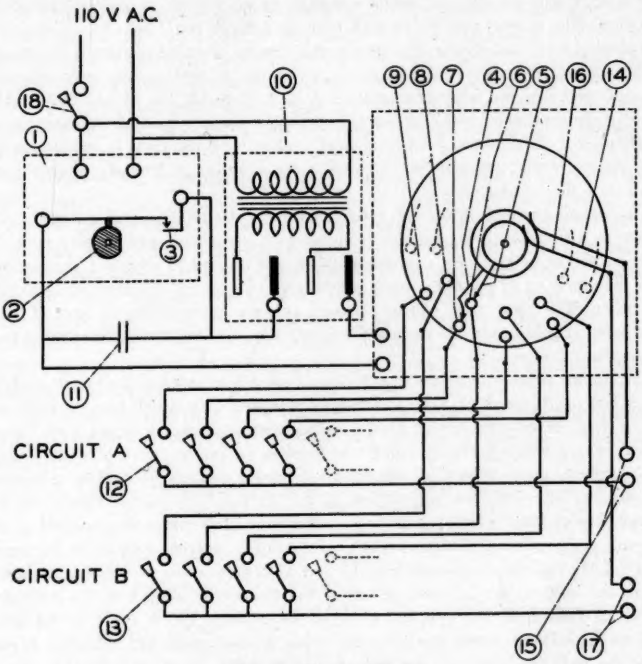


FIG. 1. TIMING RELAY
Circuit-diagram. See text for description.

automatic means. The device described here meets at least some of the principal requirements, since it is pre-set and operates automatically, is supplied entirely by A.C., and is built of stock parts.[1] It consists essentially of a stepping relay actuated at intervals of 1 sec. by a synchronous timer; it thus becomes a program-clock which operates in cycles of seconds

[1] It is designed to replace an apparatus previously described; cf. W. D. Turner, Intra-serial effects with lifted weights, this JOURNAL, 43, 1931, 1-25, esp. 5.

rather than of hours. Within its limits, the apparatus controls both the duration of the stimuli and the interval between the stimuli. For example, it may actuate electrically a standard stimulus which lasts 1 sec., then insert 2 sec. without stimulation, then actuate a comparison-stimulus lasting 1 sec., and then insert 8 sec. without stimulation, thus making in all a 12-sec. cycle.

The apparatus provides for either a contact or no contact in each second of its operation. The stimuli may be applied without a break for 1 sec. or any multiple of 1 sec. up to 47 sec. The stepping relay has 48 sets of contacts around its circumference and rotates continuously; accordingly, it may present continuously recurring temporal cycles of the following lengths: 2, 3, 4, 6, 8, 12, 16, 24 and 48 sec. The 12-sec. cycle mentioned above is convenient for the various methods of comparative judgment. For the method of single stimuli, a 6-sec. or 8-sec. cycle is usually preferable. Naturally the apparatus provides for a wide variety of irregular sequences in addition to the regular ones.

The stepping relay controls two synchronized but electrically independent circuits, so that it is possible to present one stimulus with one circuit and another with the other circuit, or to present a stimulus with one circuit and to actuate a chronoscope with the other, or to present a ready-signal with one circuit and the stimulus with the other. The relay may control circuits in two experiments at once if each experiment requires only one circuit. Obviously the relay has applications other than psychophysical ones.

The timing element, marked (1) in Fig. 1, is a synchronous clock-motor which has a final shaft-speed of 1 r.p.s. It operates a cam (2) which closes briefly the contact (3). When this contact closes, the current actuates a solenoid (not shown in the figure) which serves to rotate the brushes (4 and 5) 1/48 of a revolution, and thereby to move them from one set of stationary contacts (6 and 7) to another (8 and 9). Fig. 1 shows only a few of the 48 sets of contacts. Power for the solenoid comes from a transformer-and-rectifier unit (10) which is connected to the 110 volt A.C. main. A condenser (11), of 2.0 mfd. capacity, suppresses the arc at contact (3); and 96 toggle-switches (12 and 13), only a few of which are shown, permit the independent selection of any of the 48 contacts in each of the 2 circuits. Circuit A runs from the switches (12) to the contacts (7, 9, etc.), to the brush (4), to the collector-brush (14), to the output binding posts (15). Circuit B runs from the switches (13) to the contacts (6, 8, etc.), to the brush (5), to the collector-brush (16), to the output binding posts (17).

The cam (2) of the synchronous timer is a brass cylinder with one side filed flat. The entire contactor, including the contact-arm, its tension-spring and the contacts, is taken from the distributor-mechanism of an automobile ignition-system.

The power-supply (10) must be able to deliver 6 v., 2.2 amp. of rectified A.C.; it may be an external unit, purchased complete, or the transformer and rectifier may be housed with the other parts in a standard metal radio cabinet 19 x 13 x 8¾ in.

The design requires no chassis; all parts are fastened directly to the cabinet or panel. The 96 switches are mounted in a standard aluminum panel 19 x 8¾ in. in 6 rows of 16 each. The rows are 1¼ in. apart and the switches are 1 in. apart in each row. The power switch (18) and the output binding posts (15 and 17) are

mounted in a vertical column at the right end of the panel. Since the position of the handles on the toggle-switches indicates which switches are closed and which are open, it is advisable to choose switches which have long handles and a wide angle of throw. The order of the switches on the panel should correspond to the order of the contacts on the relay: the top row of switches controls contacts 1-16 of circuit A, the next row controls contacts 17-32, and so on. Circuit B begins with the 4th row of switches. It is accordingly quite easy to read from the panel the timing program for which the relay is already set, and to change it to a new setting.

The solenoid is somewhat noisy in operation; the noise can be reduced by mounting the stepping relay on a base of fiber-board and sponge-rubber which is in turn firmly fastened to the metal case. In any event, it is advisable to keep the whole apparatus in some central location outside the experimental rooms.

The stepping relay[2] is specified as type S-20 for 6 volt D.C. with two rows of 48 contacts each and two commutator-fingers. The fingers should be wide enough to bridge adjacent contacts.

The design of the apparatus as a whole is open to modification in several ways. With the use of other models of the stepping relay, either the number of contacts around the circumference or the number of concentric rows of contacts may be varied. Each row provides a separate output circuit; if each circuit were to control a given comparison-stimulus, the apparatus could determine, not only the timing of the stimuli, but also the order in which the stimuli are to be applied. The period of the synchronous timer may be changed so that it operates the stepping relay more frequently than 1/sec., or less frequently. Finally, a time-delay relay may be placed in an output-circuit, so that the duration of contact becomes adjustable within the limits of this relay.

The parts of the apparatus, including the stepping relay, will cost approximately $55.00. While the construction requires almost no skilled labor, it does require considerable time.

[2] Manufactured by the Guardian Electric Mfg. Co., 1621 W. Walnut St., Chicago, Ill.

# APPARATUS NOTES

## The Cornell Optical Laboratory

The high percentage of cases of aberrant color vision in every University population, the growing recognition of the significance of variations in night vision, and the consequent demand for diagnosis and advice laid upon psychologists calls for the installation and use of the most approved devices for the standardization of illumination and the control of light and color stimuli.

In default of a University department of physiological optics, a corner of the fourth floor of Morrill Hall was set aside for color vision research at the time of the remodelling of the Laboratory of Psychology. Here a dark room and a gray room suitable for the investigation of either daylight or twilight and night vision were outfitted.

Ceiling and walls are black or neutral gray, approximating Hering 7-8 or Munsell 6. Gray screens and gray or black curtains permit the cutting off of a central gray booth for routine tests with an illumination of 10 to 20 f.c.; a second booth with curved campimeter suitable for perimetry, light adaptation, or color-naming; and a third arranged for experiments in dark adaptation. Uniformity of illumination is secured by use of Corning daylight roundels chosen with the aid of spectrophotometric tests in the Physics Laboratory to approximate, when used with 200-w. bulbs, the color temperature of average daylight or a thinly overcast sky. Though the loss of efficiency of illumination is 85-90%, with corresponding heat production, these yield a closer equivalent to the solar spectrum than the so-called 'sensation daylight' filters, which may pass a preponderance of low frequency radiations and lack others entirely. A closer approximation to the daylight values of colored surfaces is thus assured, as well as a more satisfactory illuminant for light adaptation.[1] In addition to three hinged ceiling lights, there are two pairs of floor standards adjustable in height, and a 500-w. suspended unit, with rheostat and voltmeter control.

Under a National Research Council grant-in-aid a Bausch and Lomb constant deviation spectrometer with calibrated drum and screw and shutter, by means of which a color field of any wave length position from 400 to 800 mμ may readily be isolated, has been installed, and the necessary alterations for psychological observations made. A Nela ribbon filament lamp of known color curve is used as source; and a set of reduction filters made and calibrated in the Rockefeller laboratory gives the experimenter command of wide range of intensities, without alteration of slit-width or voltage. A 1-w. neon bulb provides bright lines for setting the shutter pointer, and also low illumination for adjusting voltage, manipulating filters and exposure slides, and reading experimental keys. Small bulbs illuminate the wave-length drum and slit scales. Wratten and Corning color filters are used to cut out stray light. A color square spanning 3-18 mμ at different points of the spectrum, representing a visual angle of about 3°, and centered in a circle defined

[1] Corning daylight roundels have been in use in the Cornell Laboratory of Psychology since 1916, to avoid the distortion of hues under artificial lighting. Cf. A. J. Brown, Some uses of artificial daylight in the psychological laboratory, this Journal, 27, 1916, 427-429.

by four 1-mm. points of radium paint, is usually used in experimentation.

With this instrument the range of wave length visibility, the shortening or lengthening of the violet or red end of the spectrum, both in normal and in anomalous color vision, may be explored. Color thresholds for any region of the spectrum, and the shift in hue of the non-primary colors with alterations in intensity may also be studied. Artificial pupils of 1-4 mm. diameter may be attached to the shutter. The laboratory spectroscope has also been calibrated with the aid of neon lines for use in conjunction with the spectrometer.

A color-mixer supplied with Munsell, Hering and Heidelberg colored and graduated cut-out gray disks of equated brightness permits a crude determination of the Rayleigh equation, color limens, and the shift of hue with admixture of white or black.

For the investigation of the speed of dark adaptation, an instrument has been devised on the model of the Nagel adaptometer, with central Aubert diaphragm having an areal range of 1-10,000 sq. mm., with corresponding variation in light intensity, and an illumined logarithmic scale. The source lamp is screened with a Corning daylight filter (in place of the blue glass screen of the older instrument). Flashed opal is used at back, front and behind the diaphragm to secure diffusion. The intensity range is extended by the use of neutral Eastman filters transmitting 0.1 to 10%, inserted through a trap door of the 24-in. box. These replace the cumbersome perforated screens of dubious calibration of the older instrument. A 1-in. equilateral triangle centered within four 4-mm, disks of radium paint (defining the corners of a 6-cm. square) has been adopted as the most satisfactory stimulus form for the 15-in. distance chosen for foveal adaptation trials.

This instrument may also be used for determining color limens in photopic or scotopic vision by inserting color filters behind the diaphragm. For this purpose, the rear plate of daylight glass and the front and middle screens of flashed opal may be removed or replaced with ground glass. The effect of areal variation on color, with a visual angle ranging from 1-15°, may then be studied by adjusting the diaphragm. The intensive limens for color may be determined with fixed diaphragm, varying intensity with reduction filters. Color filter combinations have been selected with the aid of the spectrometer, to secure as narrow wave length bands as possible, centering on 485, 505, 575, and 700 mμ. The socket in the lamp-box is adjustable, allowing the use of lamps of different wattage. The old inflexible type of adaptometer (*i.e.* the Nagel) is available in the graduate laboratory for comparison.

A combination curved-surface campimeter and perimeter, illumined by a pair of daylight lamps, is used for the exploration of variations in peripheral vision, with either spectroscopic light or Munsell colored paper stimuli. A complete set of the latter (about 400) is available in 1½-in. squares for experimentation; also the 1929 abridged edition of the Munsell Book of Color, for reference and classification.

A small Weston lightmeter (range 1-50 f.c.) serves to measure the illumination of test booths or walls. No photometer adequate to measure the low intensity of the light output of the spectrometer or the adaptometers has yet been found, though many experts have been consulted and many models now on the market tried out. Such an instrument must gauge accurately hundredths of a foot-candle, with either monochromatic or heterogeneous sources. The department of Electrical Engineering is collaborating on the solution.

The optical laboratory is further equipped with the standard clinical color vision tests—Nagel, Stilling, Edridge-Green, Holmgren, Ishihara—with opticians' charts as well, and lenses for the detection and correction of astigmatism, refractive defects, etc. A group test for general diagnosis has also been devised by the writer, turning on the classification of thirty-six 4-in. color disks under one or other of seven standards displayed simultaneously. A blank with stencil key is used. This is duplicated in a small individual test made up of mounted color skeins, and designed to replace Holmgren.

Another test for the detection of slight deviations from normal color sensitivity calls for the arrangement in a graded circle of twenty slightly desaturated Munsell hues of equated brightness and saturation (4/4)—a feat beyond the scope of the average dichromat. Other test material is designed to bring out deviations from the normal in the matter of color contrast, or in the relative brightness of different hues.

Apart from the theoretical aim of the correlation of degrees of night vision with degrees of color vision, the principal objective of the laboratory at present is the evolution of a relatively simple technique with standardized apparatus by means of which norms for the average eye may be established, and cases of aberrant color vision recorded for comparison in different laboratories throughout the country—for the information both of geneticists and physiologists, and vocational counsellors. A secondary aim is the correlation of standardized spectroscopic tests with filter or paper tests more informative than the present commercialized clinical ones.

The optical laboratory, however, may be utilized not only for research, but for acquainting student subjects with the precise nature of the difficulties the color deviate is apt to encounter in chemistry, botany, and various other specialized vocations. Among the cases reporting to the writer in recent years are not only the usual run of scoterythrous ranging into photerythrous dichromatism in the male, but also the rarer instances of women with color systems reduced to blue-yellow—or in one instance to yellow alone; a case of traumatic hemiopia with blue-green confusion; and a case of apparent red-green blindness accompanying mild encephalitis.

Cornell University ELSIE MURRAY

# NOTES AND DISCUSSIONS

## The Electro-Encephalogram and Its Use in Psychology

One of the problems of the psychologist is the determination of the physiological mechanisms on which psychological phenomena depend.[1] Since the brain is a dominant factor in the control of psychological activities, one phase of this problem becomes that of determining the nature of the brain-activities which regulate psychological performance.

The fact that electric potentials occur in the living brain has been known for about 60 years, and research upon such phenomena has taken place intermittently

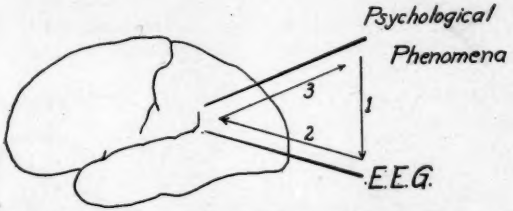


Fig. 1. Diagram Indicating the Relationship between the Psychological and the Electro-Encephalographic Phenomena

The arrows represent steps required in the use of EEG as a psychophysiological indicator.

throughout this period.[2] The present wave of interest, however, arises largely from the report by Berger in 1929, that fluctuating potentials from the human brain can be detected with electrodes attached to the scalp, without the need of direct exposure of the cortex. The name given by Berger to records thus obtained was the electro-encephalogram, or briefly, the EEG.[3]

Since the time of Berger's report, almost ten years ago, a large amount of research has been performed in laboratories and clinics throughout the world; by neurologists, psychiatrists, physiologists and psychologists. The particular question which interests us, as psychologists, is the extent to which this work contributes to the solution of our psycho-physiological problem. Specifically, is research upon the electric potentials of the brain likely to add to our knowledge of the brain-mechanisms on which

[1] From the Department of Research, The Training School at Vineland, New Jersey. This paper was presented at the meeting of the Eastern Psychological Association in New York, April 2, 1938.

[2] The following papers are illustrative: R. Caton, The electric currents of the brain, *Brit. Med. J.*, 2, 1875, 278; F. Gotch and V. Horsley, On the mammalian nervous system, its functions, and their localization determined by an electrical method, *Philos. Trans. Roy. Soc. London*, 182 B, 1891, 267-526; W. W. Neminski, Ein Versuch der Registrierung der elektrischen Gehirnerscheinungen, *Zbl. f. Physiol.*, 27, 1913, 951-960.

[3] H. Berger, Über das Elektrenkephalogram des Menschen, I, *Arch. f. Psychiat. u. Nervenkr.*, 87, 1929, 527-570.

psychological phenomena depend? If the technique in question can contribute to the solution of this problem, it is of value psychologically, otherwise it is not. My chief interest, in the present paper, will be to survey the results of research upon brain potentials in relation to this special problem of the psychologist.[4]

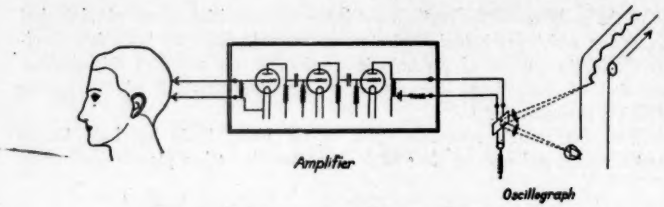


FIG. 2. APPARATUS REQUIRED IN RECORDING THE ELECTRO-ENCEPHALOGRAM

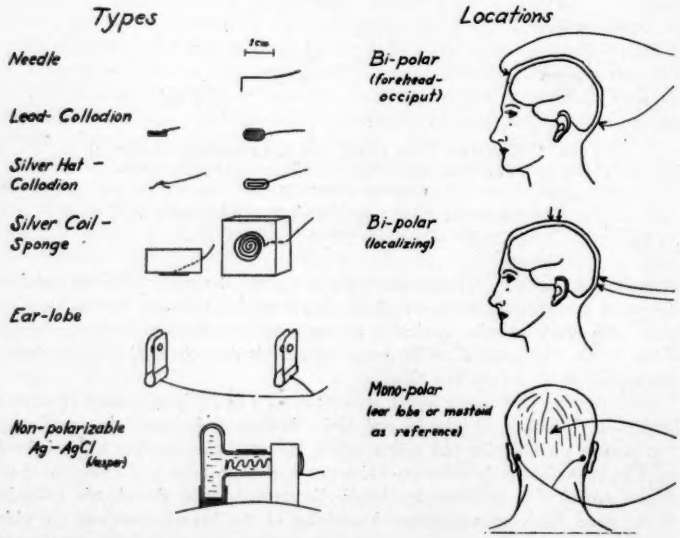


FIG. 3. ELECTRODES USED IN RECORDING HUMAN ELECTRO-ENCEPHALOGRAM
On the left are shown different types of electrodes; on the right, different ways of arranging the electrodes on the head.

*Methodological basis.* In order to obtain a guide for this survey, let us examine the methodological basis on which must rest the psychological use of the brain-

[4] No attempt is made in the present paper to give an exhaustive survey of the literature. A thorough review of the literature up to 1937 has been given by H. H. Jasper, Electrical signs of cortical activity, *Psychol. Bull.*, 34, 1937, 411-481.

potential technique. The diagram of Fig. 1 represents the assumption that both psychological phenomena and the EEG are functions of events in the brain. If the particular factors in the brain on which the EEG depends are also factors in psychological phenomena (or if the two types of factor have similar properties), then the EEG might be of use as an indicator of the physiological mechanisms underlying psychological phenomena. On the assumption that such common factors may exist in the brain, what steps are required in the use of the EEG as a psycho-physiological indicator? The following steps would seem necessary: (1) determining whether a significant empirical correlation obtains between any given psychological variable

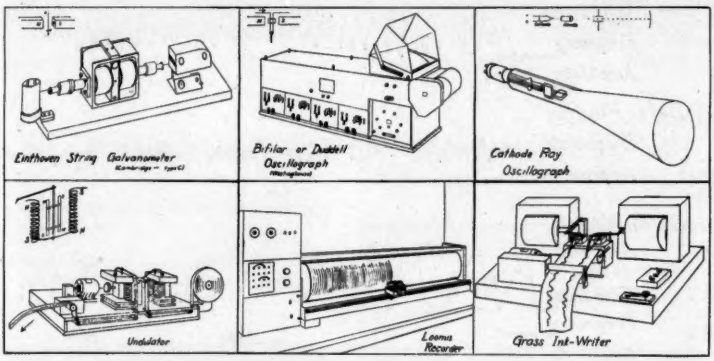


FIG. 4. TYPES OF OSCILLOGRAPH USED IN RECORDING THE ELECTRO-ENCEPHALOGRAM
Photographic oscillographs are shown in top row; ink-writing oscillographs in bottom row.

and some property of the EEG; (2) in case such an association is found, determining the factors in the brain responsible for the particular type of EEG obtained; and (3) formulating a working hypothesis of the rôle played by these factors in the psychological phenomenon under consideration.

This schema may be used as a basis for the present paper. I shall, however, first review the chief directions of research upon the physiological mechanisms responsible for the EEG. Then, on the basis of this material, we may pass to the psychological part of our survey, and consider the extent to which it has been possible to take the three steps involved in the use of the EEG as an indicator of psycho-physiological mechanisms. Before proceeding in this way, a few preliminary questions must be considered.

*(a) Recording the EEG.* Fig. 2 shows the type of equipment required: electrodes for picking up differences of potential, an amplifier for increasing their magnitude, and an oscillograph for recording them. Fig. 3 shows diagrams of different kinds of electrodes, and ways of connecting them: the bipolar and mono-polar methods. Diagrams of different kinds of oscillograph are shown in Fig. 4. In the upper row are represented oscillographs which make use of photographic recording; in the lower row, ink-writing oscillographs.

*(b) The chief phenomena of the EEG.* Fig. 5 shows some of the chief phenomena:[5] the alpha rhythm, which has a frequency of about 10 waves per sec., the beta rhythm, with an average frequency of about 25 waves per sec.,[6] and the delta waves, which are waves of long duration, longer, for example, than 1/6 of a sec.[7]

*(c) Origin of the EEG.* What is the evidence that the EEG is of cortical origin? The evidence comes from two directions. First, it must be shown that the EEG is *not* due to artifacts; secondly, more direct physiological tests of the cortical origin of the EEG may be made.

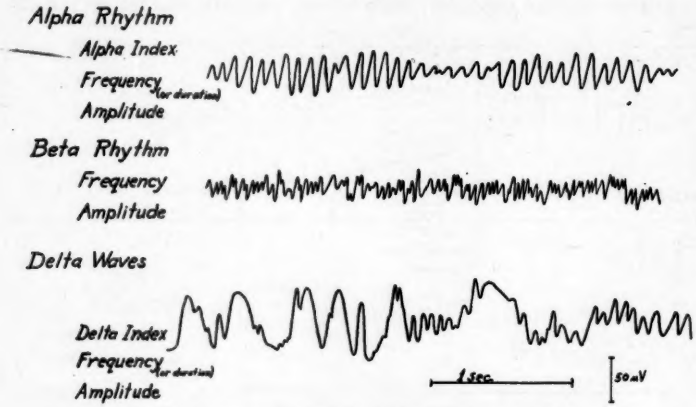


FIG. 5. CHIEF TYPES OF WAVES THAT MAY APPEAR IN THE HUMAN ELECTRO-ENCEPHALOGRAM

Under the name of each type of wave is indicated the chief properties of that wave-type which have been investigated.

Fig. 6 shows some of the different types of artifact that may appear in the records. An important part of any study will be the steps taken to exclude such artifacts or to permit their identification, when they do appear. There is strong evidence, however, that EEG records such as those shown in Fig. 5 can be obtained when these and other conceivable types of artifacts are excluded.[8]

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[5] In order to indicate the source of the records used in the various figures, the following symbols are used. An "R" placed beside a record indicates that it has been reproduced from the paper of the investigator whose work is being discussed; a "D" indicates that the record was constructed diagrammatically on the basis of the description given by the investigator of his results. Records without such symbols were obtained by the writer of the present paper. Thanks is expressed to Mr. Franklin W. Smith for assistance in the preparation of the figures used in this paper.

[6] Berger, *op. cit.* For a tabular summary of the properties of different wave-types, see H. Davis, The electroencephalogram, *Tabulae Biologicae,* 1938, in press.

[7] W. G. Walter, The location of cerebral tumours by electro-encephalography, *Lancet,* 232 (2), 1936, 305-308.

[8] E. D. Adrian and B. H. C. Matthews, The Berger rhythm: Potential changes from the occipital lobes in man, *Brain,* 57, 1934, 371-375; Berger, *op. cit.;* H. Berger, Über das Elektrenkephalogram des Menschen, III, *Arch. f. Psychiat. u. Nervenkr.,* 94,

By way of positive tests, Fig. 7 illustrates types of experiment which show, by physiological means, that potentials of cortical origin may be detected by electrodes connected to intact scalp. In an experiment by Tönnies,[9] represented in Fig. 7 A; records were obtained simultaneously from the scalp and from the cortex. Though there was a large difference in amplitude, the two records were of the same form.

The question then arises: How do we know that the potentials picked up by electrodes on the cortex originate in the cortex and not in other parts of the brain? An experiment by Dusser de Barenne and McCulloch provides an answer:[10] succes-

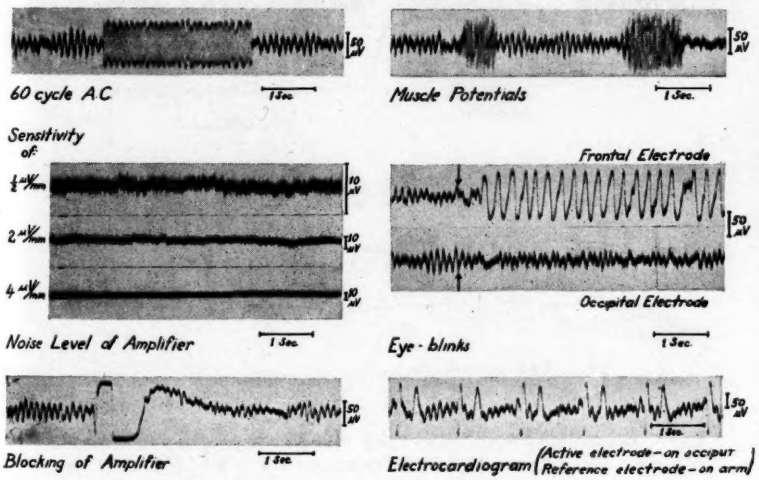


FIG. 6. SOME OF THE ARTIFACTS THAT MAY APPEAR IN THE ELECTRO-ENCEPHALOGRAM

sive layers of the cortex were destroyed by heating; corresponding changes appeared in the electrical records picked up from the cortex, until all signs of electric activity disappeared after the last layer was destroyed (Fig. 7 B). The cortex itself must therefore be regarded as the source of the potentials originally detected from intact cortex.[11] This experiment, taken in conjunction with that of Tönnies

1931, 16-61; H. H. Jasper and H. L. Andrews, Human brain rhythms: I. Recording techniques and preliminary results, *J. Gen. Psych.*, 14, 1936, 98-126.

[9] J. F. Tönnies, Die Ableitung bioelektrischer Effekte vom uneröffneten Schädel, *J. f. Psychol. u. Neurol.*, 45, 1933, 154-171.

[10] J. G. Dusser de Barenne and W. S. McCulloch, Some effects of laminar thermocoagulation upon local action potentials of the cortex of the monkey, *Amer. J. Physiol.*, 114, 1936, 692-694.

[11] The records reproduced in Fig. 7 B are intended, in the present context, to demonstrate only that the potentials picked up from the intact cortical surface originate in the cortex itself. A further question may be raised concerning the particular cortical layers in which the different rhythms originate. Berger, for example, has drawn the inference from the experiment of Dusser de Barenne and McCulloch that the rela-

demonstrates that electric potentials of cortical origin may be detected by electrodes attached to the scalp.

*Physiological mechanisms.* The bulk of the work performed to determine the physiological mechanisms responsible for the EEG may be summed up as answers to four questions. The fundamental question concerning the place of origin of the

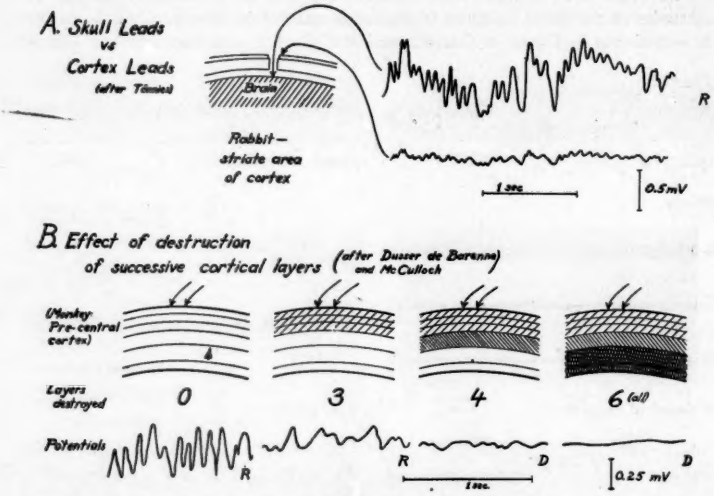


FIG. 7. EXPERIMENTS INDICATING THAT ELECTRIC POTENTIALS OF CORTICAL ORIGIN MAY BE DETECTED ON THE SCALP

EEG has just been considered. A second question is: How are the cortical rhythms initiated?

*(a) Initiation of cortical rhythms.* Do the cortical rhythms depend upon afferent impulses reaching the brain? Decisive evidence upon this question has been furnished by Bremer.[22] He found, in the cat, that blocking the flow of afferent impulses by

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tively slow alpha rhythm arises in the deeper layers of the cortex, and the faster beta rhythm in the outer layers (H. Berger, Über das Elektrenkephalogram des Menschen XII, *Arch. f. Psychiat. u. Nervenkr.*, 106, 1937, 165-187). Dusser de Barenne and McCulloch do not, however, accept this interpretation. To obtain more decisive information on the question, they performed experiments with chronic preparations (J. G. Dusser de Barenne and W. S. McCulloch, Kritisches u. Experimentelles zur Deutung der Potentialschwankungen des Electrocorticogramms, *Zsch. f. ges. Neurol. u. Psychiat.*, 1938, in press). After thermocoagulation of all except the inner two cortical layers (motor area of monkey), sufficient time was allowed (6 weeks) for the killed tissue to be absorbed. The higher frequency fluctuations, as well as those of lower frequency, were still present in the electrical record obtained from the inner two layers, a result which fails to confirm Berger's hypothesis. The authors point out the reasons why differences are to be expected in the records from the inner layers in chronic and acute experiments.

[22] F. Bremer, Cerveau "isolé" et physiologie de sommeil, *C. r. soc. biol.*, 118, 1935, 1235-1241; Quelques propiétes de l'activité électrique du cortex cérébral "isolé," *ibid.*, 1241-1244.

section across the mid-brain did not bring about the disappearance of cortical rhythms (Fig. 8 A). There occurred, instead, a considerable increase in the amplitude of the waves, associated with a decrease in frequency (from a frequency of 25 to 30 waves a sec. to one of 10 to 15 waves a sec.). Experiments by Gerard on the isolated olfactory lobe of the frog,[13] and by Bishop[14] on the cortex of the rabbit, cut off from impulses from the thalamus, likewise indicate that afferent impulses are *not* required to initiate cortical rhythms. The brain is not a passive organ that merely reflects sensory stimulation, but is the seat of continuous spontaneous activity.

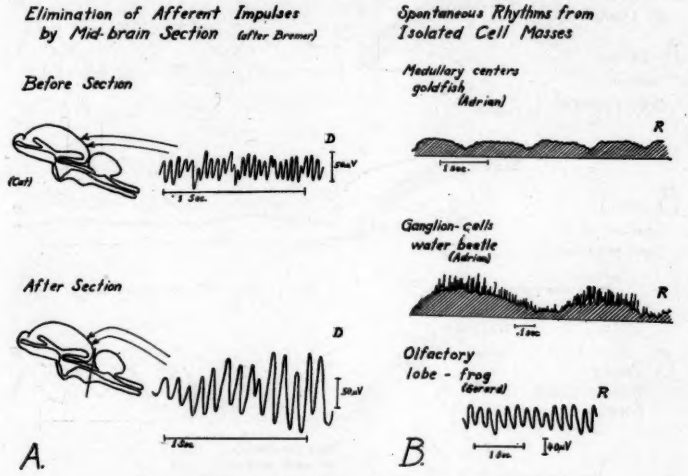


FIG. 8. THE INITIATION OF CORTICAL RHYTHMS

(A) Experiment showing that cortical rhythms may appear in the absence of afferent impulses to the brain; (B) Records showing that spontaneous rhythms may be detected in isolated masses of nerve-cells.

How, then, are the cortical rhythms initiated within the brain? In answer to this question two hypotheses have been proposed. One is that the rhythms are initiated within the cortical cells themselves. The chief evidence for this doctrine lies in the array of experiments which shows that electrical rhythms may occur in *isolated* cell masses.[15] Fig. 8 B shows a number of examples. This evidence indicates there is no

[13] R. W. Gerard and J. Z. Young, Electrical activity of the central nervous system of the frog, *Proc. Roy. Soc. London*, B828, 1937, 343-352.

[14] G. H. Bishop, The interpretation of cortical potentials, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 305-319.

[15] For a discussion of such experiments, see C. L. Prosser, Rhythmic activity in isolated nerve centers, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 339-346, and R. W. Gerard, Factors controlling brain potentials, *ibid.*, 4, 1936, 292-304. The third record in Fig. 8 B was taken from R. W. Gerard, *ibid.* The first two records were taken from E. D. Adrian, *The mechanism of nervous action*, 1932, 78-94, by permission of the University of Pennsylvania Press.

theoretical reason why rhythms may not be initiated, in like manner, in the cells of the cerebral cortex. A second hypothesis holds that certain cortical rhythms depend on closed neurone-chains extending to the thalamus. An experiment of Bishop and Bartley lends strong support to this doctrine. As indicated in Fig. 9 A, a region of the rabbit's cortex is almost completely encircled by incisions, but with fibers from the thalamus to the cortex left intact. The electrical record obtained from the cortex shows two main rhythms, a slow rhythm with a frequency of about 5 waves a sec., and

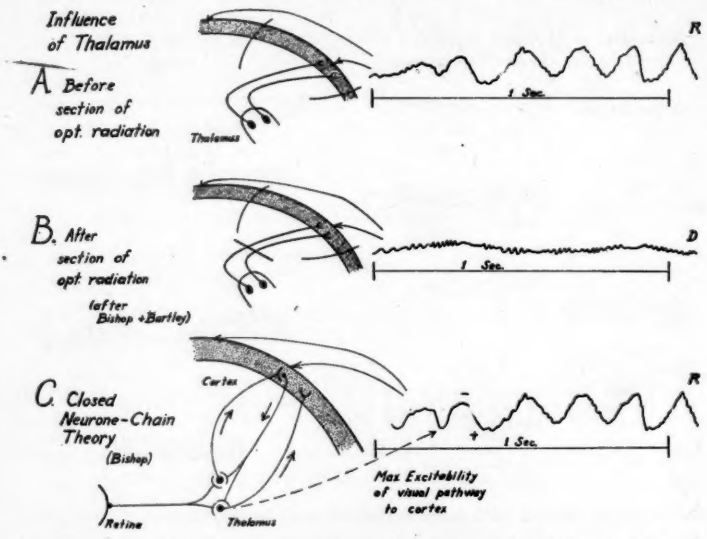


FIG. 9. DIAGRAMS ILLUSTRATING THE RÔLE PLAYED BY THE THALAMUS IN MAINTENANCE OF CORTICAL RHYTHMS

(After Bishop)

another, of much higher frequency. When the fibers from the thalamus are now cut (Fig. 9 B), the slow cortical rhythm is lost, but the fast rhythm remains.[38] The inference is that impulses from the thalamus are necessary for maintenance of the slow rhythm, whereas the faster rhythm apparently arises in the cortical cells themselves. Bishop suggests the theory, illustrated in Fig. 9 C, that the slow rhythm is maintained

[38] Bishop, *op. cit.* In his most recent experiments on this problem, Bishop finds that the 5 per sec. rhythm is often replaced, when the fibers from the thalamus are cut, by sequences of still slower waves, about 1 to 1½ sec. in duration. This result, too, indicates that the normal 5 per sec. rhythm depends on the thalamus for its maintenance. Bishop suggests that the extremely slow waves which may appear after section of the thalamic fibers probably arise in the cortex spontaneously, and that in the normal case (fibers from the thalamus intact), these slow spontaneous waves are speeded up to a 5 per sec. rate by the thalamic circuit (Personal communication).

by impulses travelling around a closed circuit from thalamus to cortex and back again.[37]

*(b) Organization of cortical activity.* How do the cortical rhythms depend on the organized activity of cells in the cortex? This question has aroused considerable dis-

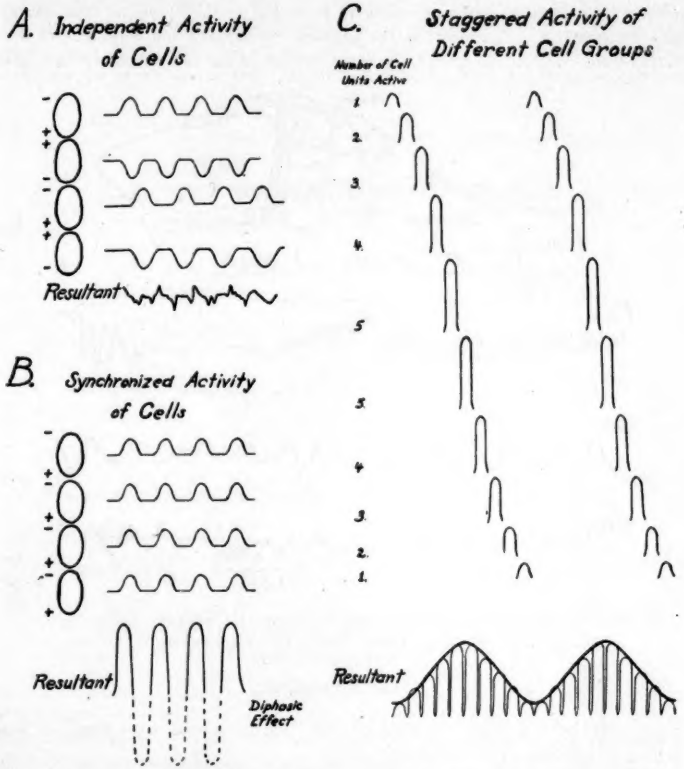


FIG. 10. THE ORGANIZED ACTIVITY OF CORTICAL CELLS

Diagrams illustrate how the resultant electrical potentials recorded from a group of cells may depend on the relations of activities in the component cells.

cussion. Fig. 10 illustrates some of the chief possibilities. Diagram A of Fig. 10 shows that if each of the cells in a group is beating independently of the others, the resultant effect is likely to be a series of fluctuations of low amplitude, and no evident rhythm. The resultant curve is different from any of the components, and the fre-

[37] Bishop, *op. cit.*

quency of discharge in individual cells may *not* be inferred from the resultant curve. Fig. 10 B illustates a group of cells beating with the same frequency and in time with each other. The resultant record for the group shows the same frequency as the component cells, but with greatly increased amplitude. A third possibility is illustrated in Fig. 10 C. Different groups of cells discharge in sequence and in a manner to build up a resultant curve of sine-wave form, but of longer period than any of the components. By adjustments in the temporal overlapping and amplitude of discharges from different cell-groups, the characteristics of the component waves might,

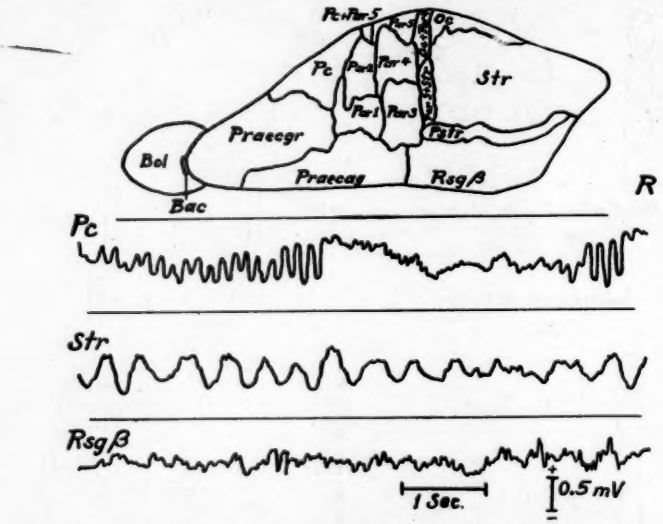


FIG. 11. RECORDS OF THE ELECTRIC POTENTIALS DETECTED FROM DIFFERENT CYTO-ARCHITECTONIC AREAS OF THE CEREBRAL CORTEX OF THE GUINEA PIG

(From Kornmüller)

conceivably, by completely obscured and only the modulated wave form, of long period, be apparent in the resultant record.

What now are the chief views held regarding the organized activities of the cells? It is quite generally agreed that the cells of the cortex are beating together in organized fashion. Whether the organization is that of absolute synchrony, or whether it corresponds to a stagger arrangement is still regarded as an open question. There is also disagreement concerning the duration of the individual electrical discharges in single cells. Is the duration as short as the action potential of nerve fibers (about 1 millisec.); or is it as long as single alpha waves; or is it of intermediate duration? Each of these views is still regarded as possible.[25] We now

[25] A discussion of different points of view may be found in papers and discussions of papers by Bishop, Davis, Gerard, Jasper, and Prosser in *Cold Spring Harbor Symp. Quant. Biol.*, 4, 1936, 285-346. See also E. D. Adrian and B. H. C. Matthews,

pass to the fourth question: How are the cortical potentials affected by factors known to affect brain-cells?

*(c) Influence of factors known to affect brain-cells.* Four types of factors may be considered: the structure of the cells, sensory stimulation, the physical-chemical environment of the cells, and the activity of other brain-cells. Let us consider first the influence of *cell-structure.* Fig. 11 illustrates results of studies by Kornmüller.[19] The top figure shows the different areas of the guinea-pig's cortex, differentiated on the basis of the structure of the cells in different cortical layers. Underneath is shown the nature of the electrical records obtained from a number of different

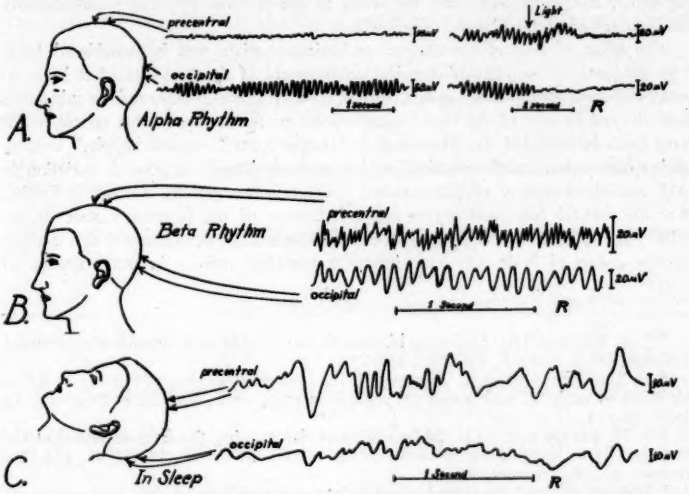


FIG. 12. THE DETECTION, IN MAN, OF SPECIFIC ELECTRICAL EFFECTS WITH ELECTRODES LOCATED OVER DIFFERENT CORTICAL AREAS
(After Jasper and Andrews)

areas. Kornmüller's conclusion was that the structure of the cells in a given region is a definite factor in the type of record obtained. Although some experimenters are not in complete agreement with this view,[20] the evidence on the whole indicates a variation of the electrical pattern with cell-structure.

It has not been possible to investigate *in man* the variation of the electro-encephalogram with brain area as directly as is the case with animals. The layer of cerebro-spinal fluid between the brain and the electrodes on the scalp diffuses out the potentials arising in a given cortical area, and permits their detection

The Berger rhythm: Potentials changes from the occipital lobes in man, *Brain,* 57, 1934, 371-375.

[19] A. E. Kornmüller, Die bioelektrischen Erscheinungen der Grosshirnrinde, *Fortsch. Neurol.,* 5, 1933, 419-441.

[20] E. D. Adrian, The spread of activity in the cerebral cortex, *J. Physiol.,* 88, 1936, 127-161; M. B. Rheinberger and H. H. Jasper, The electrical activity of the cerebral cortex in the unanesthetized cat, *Amer. J. Physiol.,* 119, 1937, 186-196.

over a relatively wide region of the scalp.[21] Evidence has been found, however, even in records obtained from the scalp, of the production of specific electric effects by different cortical areas. Fig. 12 illustrates some results of a study by Jasper and Andrews on this question.[22] Diagram A of Fig. 12 indicates that although alpha rhythms may be obtained over both occipital and precentral areas, they do not come from the same *locus* in the brain, since the alpha rhythm may be absent over one region when it is simultaneously present over a different region. Similar evidence was found for the beta rhythm. This rhythm is most prominent over the motor area, and is often present over this area while absent over the occiput. During sleep, also, differences may be found in the patterns obtained simultaneously over motor and occipital areas (Fig. 12C).

The effect of sensory stimulation on brain-potentials will be considered later. The influence of the physical-chemical environment of the brain-cells has been investigated both by means of agents applied directly to the cortex, and by substances administered by way of the blood stream. Some of the factors in this sphere which have been investigated are blood-supply,[23] temperature,[24] oxygen supply,[25] concentration of metallic ions,[26] narcotics,[27] and convulsant drugs.[28] In general, variation in such conditions tend to produce marked changes in the records, a result indicating that the cortical potentials are sensitive indicators of the functional state of the cells. Finally, observations by a number of workers have demonstrated that activity in one group of brain-cells may influence electrical activity in other groups of cells.[29]

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[21] J. F. Tönnies, Die Ableitung bioelektrischer Effekte vom uneröffneten Schädel, *J. f. Psychol. u. Neurol.*, 45, 1933, 154-171.

[22] H. H. Jasper and H. L. Andrews, Electro-encephalography: III. Normal differentiation of occipital and precentral regions in man, *Arch. Neurol. & Psychiat.*, 39, 1938, 96-115.

[23] S. H. Bartley and G. H. Bishop, Factors determining the form of the electrical response from the optic cortex of the rabbit, *Amer. J. Physiol.*, 103, 1933, 173-184; Bremer, *op. cit.* (footnote 12)

[24] Bremer, *ibid.*; H. Hoagland, Temperature characteristics of the "Berger rhythm" in man, *Science*, 83, 1936, 84-85; Electrical brain waves and temperature, *ibid.*, 84, 1936, 139-140.

[25] R. W. Gerard, Factors influencing brain potentials, *Trans. Amer. Neurol. Soc.*, 62, 1936, 55-60; Factors controlling brain potentials, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 292-304; F. A. Gibbs, H. Davis, and W. G. Lennox, The electroencephalogram in epilepsy and in conditions of impaired consciousness, *Arch. Neurol. & Psychiat.*, 34, 1935, 1133-1148.

[26] Gerard, *ibid.*

[27] F. Bremer, Action du différents narcotiques sur les activités électriques spontanée et reflexe du cortex cérébral, *C. rend. soc. biol.*, 121, 1936, 861-866; A. J. Derbyshire, B. Rempel, A. Forbes, and E. F. Lambert, The effects of anesthetics on action potentials in the cerebral cortex of the cat, *Amer. J. Physiol.*, 116, 1936, 577-596; E. A. Spiegel, Comparative study of the thalamic, cerebral, and cerebellar potentials, *Amer. J. Physiol.*, 118, 1937, 569-579.

[28] J. G. Dusser de Barenne and W. S. McCulloch, Some effects of local strychinization on action potentials of the cerebral cortex of the monkey, *Proc. Amer. Neurol. Assoc.*, 66, 1936, 47-48; M. H. Fischer and H. Löwenbach, Aktionsströme des Zentralnervensystems unter der Einwirkung von Krampfgiften, *Arch. f. exper. Pathol.*, 174, 1934, 357-382, 502-516.

[29] G. H. Bishop, The interpretation of cortical potentials, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 305-319; J. G. Dusser de Barenne and W. S. McCul-

*Relation of psychological phenomena to the EEG.* We may now pass to the *psychological* part of our survey. The chief question that will concern us will be: Are there significant correlations between the psychological and the EEG variables? The finding of such correlations must be the first step in the use of the EEG as an indicator of psycho-physiological processes. If such associations are found, we may then attempt to follow the three-fold program outlined earlier. The psychological phenomena to be considered may be classified under two headings: psychological *activities* and psychological *capacities.*

*(1) Psychological activities. (a) Specific activities.* We may first examine studies that have been made of changes in the EEG with the initiation of *different psychological activities.*[20] Let us select a subject with a good occipital alpha rhythm

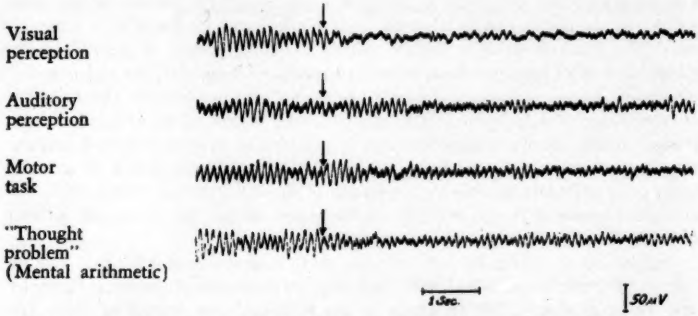


FIG. 13. CHANGES IN THE ALPHA RHYTHM UPON INITIATION OF DIFFERENT PSYCHOLOGICAL ACTIVITIES

The arrow, in the first two records, indicates onset of the stimulus; in the last two records, it indicates signal to the subject to begin task.

and determine the effect of initiating various psychological activities. Will there be associated changes in the alpha rhythm? Typical results are shown in Fig. 13. The psychological events considered are visual stimulation, auditory perception, a complex motor task, and the solution of a "thought problem." The records show a curious result. In every instance, the same general type of effect occurs—the disappearance or marked depression of the alpha rhythm. This effect may not be invariable. One supplementary condition usually necessary is that the subject not be habituated to the activity in question. Thus, a loud sound repeated for some time finally loses its effectiveness.[21]

How are we to explain the fact that the same type of change in the EEG is associated with a variety of psychological activities? Most of the workers on this

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loch, *op. cit., Proc. Amer. Neurol. Assoc.,* 66, 1936, 47-48; R. R. Grinker, A method for studying and influencing cortico-hypothalmic relations, *Science,* 87, 1938, 73-74.

[20] Most of the work upon this problem has been limited to the alpha rhythm, though there have been a few reports dealing with other properties of the EEG.

[21] B. K. Bagchi, The adaptation and variability of response of the human brain rhythm, *J. of Psychol.,* 3, 1937, 463-485.

problem have come to the conclusion that the depression of the alpha rhythm is connected in some way with the subject's state of attention.[32] As Berger pointed out, for a sensory stimulus to be effective in depressing the alpha rhythm, it must attract the subject's attention. Other lines of evidence point, in like manner, to the significance of attention. This association has its difficulties, however. The topic of attention is notorious for the many different ways in which the term is used, and for the uncertainty of its systematic status. A similar ambiguity of meaning may be found in the use of attention in the EEG studies. This situation suggests the need for a critical examination of the EEG results in relation to different concepts and doctrines of attention.

Another question raised concerning the depression of the alpha rhythm is this: How is the depression brought about physiologically? A number of hypotheses have been proposed. According to Adrian and Matthews,[33] the presence of the alpha rhythm before stimulation is taken to imply synchronized discharge in a group of cells. The visual stimulus is said to initiate a *specific* pattern of activity among these cells and so break up the synchronized action. Consequently, the alpha rhythm disappears. Berger suggests a different hypothesis.[34] He regards the alpha rhythm as a resultant of activity in a large mass of cells in the cortex as a whole. Attention to any specific sensory stimulus is regarded as associated with a facilitation of processes in one specific locus in the cortex, and the inhibition of activities in all other parts. He believes the depression of the alpha rhythm reflects this process of inhibition in the greater part of the cortex. Berger points out the relation of this doctrine to Pavlov's theory of cortical inhibition.

In addition to the results just discussed, there is an appreciable body of data on *special EEG effects* associated with particular psychological phenomena. Examples are: Effect of quantitative variations in the stimulus,[35] the control of alpha frequency by rate of visual flicker,[36] the correspondence of the alpha rhythm with the sequence of after-images,[37] the relation between cortical rhythms and muscular

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[32] Bagchi, *ibid.*; H. Berger, Über das Elektrenkephalogram des Menschen, II, *J. f. Psychol. u. Neurol.*, 40, 1930, 160-179; Über das Elektrenkephalogram des Menschen, IV, *Arch. f. Psychiat. u. Nervenkr.*, 97, 1932, 6-26; G. Durup and A. Fessard, L'électrencephalogramme de l'homme: Observations psycho-physiologiques relatives a l'action des stimuli visuals et auditifs, *Ann. Psychol.*, 36, 1935, 1-32; H. H. Jasper and R. M. Cruickshank, Electro-encephalography: Visual stimulation and the after-image as affecting the occipital alpha rhythm, *J. Gen. Psychol.*, 17, 1937, 29-48.

[33] E. D. Adrian and B. H. C. Matthews, The Berger rhythm: Potential changes from the occipital lobes in man, *Brain*, 57, 1934, 371-375.

[34] H. Berger, Über das Elektrenkephalogram des Menschen, II, *J. f. Psychol. u. Neurol.*, 40, 1930, 160-179; Über das Elektrenkephalogram des Menschen, IV, *Arch. f. Psychiat. u. Nervenkr.*, 97, 1932, 6-26.

[35] G. Durup and A. Fessard, L'électrencephalogramme de l'homme: Données sur l'arret provoqué par des stimuli visuels ou auditifs, *C. rend. soc. biol.*, 122, 1936, 756-758.

[36] E. D. Adrian and B. H. C. Matthews, The Berger rhythm: Potential changes from the occipital lobes in man, *Brain*, 57, 1934, 371-375; G. Durup and A. Fessard, L'électrencephalogramme de l'homme: Observations psycho-physiologiques relatives a l'action des stimuli visuels et auditifs, *Ann. Psychol.*, 36, 1935, 1-32.

[37] H. H. Jasper and R. M. Cruickshank, Electro-encephalography: II. Visual stimulation and the after-image as affecting the occipital alpha rhythm, *J. Gener. Psychol.*, 17, 1937, 29-48.

rhythms,[38] and changes in the alpha rhythm with conditioning[39] and during the temporal course of consciousness.[40]

*(b) Levels of activity.* A second topic that we may take up under the heading of psychological activities is that of *levels* of activity or excitation. The concept, though not very precise, helps in a preliminary condensation of results.[41] We may conceive of three general levels of psychological activity, with the basal state of relaxation regarded as a reference point. On the *positive side* of this basal state may be represented states of definite psychological activity, as present, for example, in the instances we have just been considering. On the *negative side* of the basal state

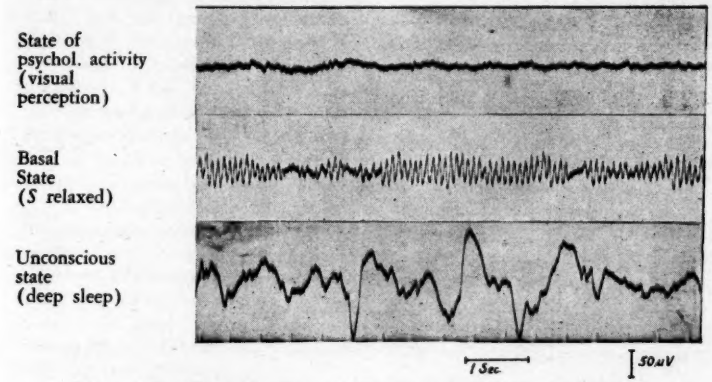


FIG. 14. THE OCCIPITAL ELECTRO-ENCEPHALOGRAM CORRESPONDING TO DIFFERENT LEVELS OF PSYCHOLOGICAL ACTIVITY

may be represented states of unconsciousness, and the intermediate states leading up to it. Fig. 14 illustrates a tentative generalization that may be made concerning the brain-potentials associated with these different levels. The records shown in the figure were all taken on the same subject. In states of activity or excitation, we find a low voltage record, with alpha waves practically eliminated, as already reported. In the basal or relaxed state, we find a marked alpha rhythm present, at an amplitude

[38] H. Berger, Über das Elektrenkephalogram des Menschen, I, *Arch. f. Psychiat. u. Nervenkr.*, 87, 1929, 527-570; H. H. Jasper, Cortical excitatory state and synchronism in the control of bio-electric autonomic rhythms, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 320-338; L. E. Travis and C. N. Cofer, The temporal relation between brain potentials and certain neuromuscular rhythms, *J. Exper. Psychol.*, 21, 1937, 565-569.

[39] A. L. Loomis, E. N. Harvey, and G. Hobart. Electrical potentials of the human brain, *J. Exper. Psychol.*, 19, 1936, 249-279.

[40] L. E. Travis, Brain potentials and the temporal course of consciousness, *J. Exper. Psychol.*, 21, 1937, 302-309.

[41] Cf. H. Davis, Some aspects of the electrical activity of the cerebral cortex, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 285-291; H. H. Jasper, Cortical excitatory state and synchronism in the control of bio-electric autonomic rhythms, *ibid.*, 320-338.

of about 70 microvolts. In the unconscious state, we find extremely large slow delta waves, with a maximum amplitude of about 280 microvolts.

In this particular record, the unconscious state was that of deep sleep. It is a striking fact, however, that the same basic type of record may be obtained in a

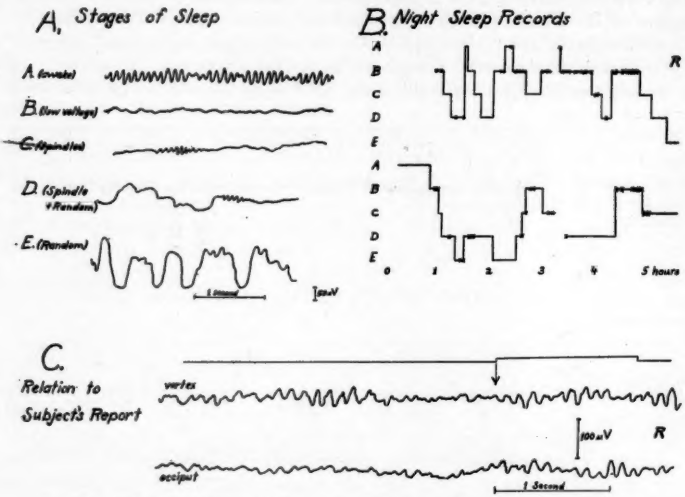


FIG. 15. ELECTRO-ENCEPHALOGRAPHIC EFFECTS ASSOCIATED WITH SLEEP

number of unconscious states, regardless of the particular way in which unconsciousness is induced. Whether the inducing factor is sleep,[42] asphyxia,[43] anesthesia,[44]

[42] H. Blake and R. W. Gerard, Brain potentials during sleep, *Amer. J. Physiol.*, 119, 1937, 692-703; F. A. Gibbs, H. Davis, and W. G. Lennox, The electroencephalogram in epilepsy and in conditions of impaired consciousness, *Arch. Neurol. & Psychiat.*, 34, 1935, 1133-1148; A. L. Loomis, E. N. Harvey and G. A. Hobart, Cerebral states during sleep, as studied by human brain potentials, *J. Exper. Psych.*, 21, 1937, 127-144.

[43] F. A. Gibbs, H. Davis, and W. G. Lennox, The electroencephalogram in epilepsy and in conditions of impaired consciousness, *Arch. Neurol. & Psychiat.*, 34, 1935, 1133-1148.

[44] Certain differences in the type of change produced in the brain potential pattern have been found associated with different types of anesthetic. Hypnotics of the barbituric acid group tend to be associated with the appearance of large, slow waves (F. Bremer, Action du différents narcotiques sur les activités électriques spontanée et reflexe du cortex cérébral, *C. rend. soc. biol.*, 121, 1936, 861-866; A. J. Derbyshire, B. Rempel, A. Forbes, and E. F. Lambert, The effects of anesthetics on action potentials in the cerebral cortex of the cat, *Amer. J. Physiol.*, 116, 1936, 577-596; E. A. Spiegel, Comparative study of the thalamic, cerebral, and cerebellar potentials, *Amer. J. Physiol.*, 118, 1937, 569-579). In ether anesthesia, on the other hand, small rapid waves may be found, especially in the lighter stages of anesthesia, in addition to large slow waves (frequency of 1 to 3 a sec.) on which they may be superimposed. The reason for such differences is as yet obscure.

or the coma of insulin shock,[46] an EEG pattern of this nature may be obtained. By way of theory, this effect might be attributed either to changed activities in the same set of cells which produce the alpha rhythm, or to a new group of cells whose activity was previously inhibited by the cells responsible for the alpha rhythm.[46] Such hypotheses suggest interesting speculations on the relation of conscious activities to one or the other set of cells.

Special EEG effects have also been found in connection with the particular way in which unconsciousness is induced. Some facts concerning sleep, for example, are summarized in Fig. 15, based upon work reported from the Loomis laboratory. Section A of Fig. 15 shows how the EEG pattern may vary in different stages of sleep.[47] Section B represents, in the case of 2 *Ss* the course of a night's sleep graphed in terms of these stages.[48] Section C is probably of greatest interest from the point of view of the psychologist. It indicates the relation between *S*'s signals while falling asleep and the EEG pattern. *S* is instructed to go to sleep and to signal just after each instance in which he notes that he has momentarily "drifted off." A close correspondence was found between the incidence of such signals and a period of depression of the alpha waves just before the signal occurs. There is thus evidence of a definite correlation between *S*'s experience and the EEG pattern.[49]

*(2) Psychological capacities.* Let us now pass to the psychological capacities, and determine what relations have been found between them and the EEG. These capacities may be regarded as quasi-permanent properties of an individual that are attributed to him on the basis of how he behaves under standardized conditions. The psychological capacities may be taken up under three headings: (*a*) the variations in the capacities at different developmental stages, (*b*) the variations found among different individuals of a particular class, and (*c*) the variations associated with pathological states.

*(a) Developmental variations.* The EEG has been followed through different developmental stages by tracing the changes occurring at different *life ages* of the individual. Lindsley[50] and Smith[51] have performed the most extensive studies on human *Ss*. The chief results are (i) the occurrence of chiefly random activity in the new born; (ii) the first apeparance of slow regular rhythms over the *motor* area within a few days after birth, and over the *occipital* area at about the age of 3 mo.; and (iii) a progressive increase in *frequency* of the occipital alpha rhythm with life age, from the age of 4 mo. until the age of 8 yr., when the frequency level characteristic of adults is reached. With respect to psychological correlation, Linds-

[46] H. Hoagland, M. A. Rubin, and D. E. Cameron, The electroencephalogram of schizophrenics during hypoglycemia and recovery, *Amer. J. Physiol.*, 120, 1937, 559-570.

[46] G. Kreezer, (Discussion), *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 289-290.

[47] A. L. Loomis, E. N. Harvey, and G. A. Hobart, Cerebral states during sleep, as studied by human brain potentials, *J. Exper. Psych.*, 21, 1937, 127-144; cf. H. Blake and R. W. Gerard, Brain potentials during sleep, *Amer. J. Physiol.*, 119, 1937, 692-703.

[48] Loomis et al., *op. cit.*

[49] H. Davis, P. A. Davis, A. L. Loomis, E. N. Harvey, and G. Hobart, Human brain potentials during the onset of sleep, *J. Neurophysiol.*, 1, 1938, 24-38.

[50] D. B. Lindsley, Brain potentials in children and adults, *Science*, 84, 1936, 354.

[51] R. J. Smith, The electroencephalogram during infancy and childhood, *Proc. Soc. Exper. Biol. and Med.*, 36, 1937, 384-386.

ley points out that the occipital alpha rhythm first appears at about the time the child becomes capable of visual perception.

The reported variation of alpha frequency with life age leaves open the question whether the decisive factor is life age, or whether it is mental age, which in normal children will vary with life age. Experiments with the feeble-minded on the relation between the EEG and intelligence level throws some light on this question.[82] The use of adult mentally deficient *Ss* permits life age to be kept, in effect, constant while mental age varies. In special types, such as the Mongolian, variation in alpha *frequency* with mental age was not found. In the hereditary type, however, a significant influence of mental age on frequency was found. Two points are worthy of special note: (1) the fact that as mental age increased in the hereditary group, an average alpha frequency that closely approximated that of mentally normal *Ss* occurred rather abruptly at a mental age of 8 yr., the life age at which alpha frequency reaches adult levels in normal children; and (2) the fact that this variation of frequency with mental age occurred specifically in the hereditary type. The hereditary type of mental deficiency has been thought of as representing the lower range of the normal curve of distribution, whereas deficiencies of types such as the Mongolian are regarded as due to special pathological factors. These results suggest that the variation in alpha frequency with life age found in normal children is a function of changing mental age rather than life age as such.

In the studies made of variations in the EEG with mental age, properties other than alpha frequency which have been examined are alpha index, alpha-wave amplitude and delta index. For certain of these properties statistically significant correlations with mental age were also found, with the relation differing in different types of mental deficiency. Tentative physiological hypotheses have been proposed to account for these results.

*(b) Individual differences.* A second field in which the variation of the EEG with psychological capacity may be examined is that of individual differences. Two outstanding facts have appeared. (1) In practically every class of *Ss*, a wide range of individual differences in the EEG is found.[83] Alpha index may, for example, range between 0 to close to 100 in normal *Ss*. Fig. 16 shows the range of individual differences in various properties of the EEG that may occur in different types of *Ss*. (2) A given *S* tends to show the same type of EEG pattern in records taken at different times.[84] The wide range of individual differences and the consequent tend-

[82] G. Kreezer, The electric potentials of the brain in certain types of the mentally deficient, *Arch. Neurol. & Psychiat.*, 36, 1936, 1206-1213; Electrical phenomena of the brain among the feebleminded, *Proc. Amer. Assoc. Mental Deficiency*, 42, 1937, 130-141, and *Train. Sch. Bull.*, 35, 1938, 4-16; The dependence of the electro-encephalogram upon intelligence level, *Psych. Bull.*, 34, 1937, 769; and unpublished data.

[83] H. Davis and P. A. Davis, Action potentials of the brain in normal persons and in normal states of cerebral activity, *Arch. Neurol. & Psychiat.*, 36, 1936, 1214-1224; H. Hoagland, E. Cameron, and M. A. Rubin, The electroencephalogram of schizophrenics during insulin treatments, *Amer. J. Psychiat.*, 94, 1937, 183-208; G. Kreezer, The dependence of the electro-encephalogram upon intelligence level, *Psych. Bull.*, 34, 1937, 769; D. B. Lindsley, Brain potentials in children and adults, *Science*, 84, 1936, 354.

[84] H. Berger, Über das Elektrenkephalogram des Menschen, II, *J. f. Psychol. u. Neurol.*, 40, 1930, 160-179; H. Davis, Some aspects of the electrical activity of the cerebral cortex, *Cold Spring Harbor Sympos. Quant. Biol.*, 4, 1936, 285-291; L. E.

ency to overlapping of different groups, emphasizes the need for statistical methods in the comparison of the EEGs of *different types* of Ss. General impressions based on casual inspection of records is not enough.

How are such individual differences to be explained? Research has run along two lines. Individual differences in the EEG have been examined in relation to *biological variables*, on the one hand, and in relation to *psychological variables*,

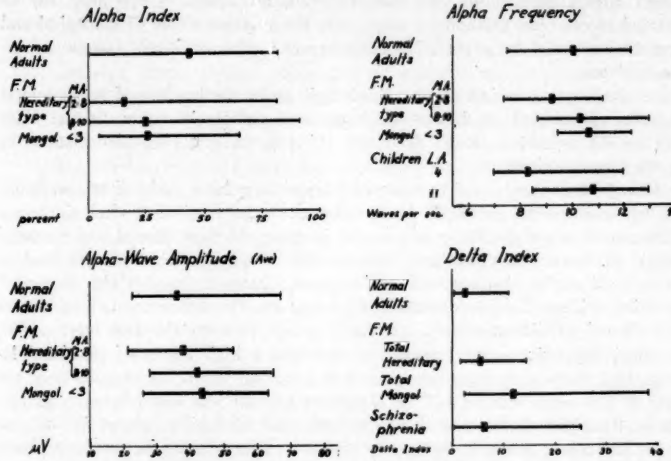


FIG. 16. RANGE OF INDIVIDUAL DIFFERENCES IN VARIOUS PROPERTIES OF THE ELECTRO-ENCEPHALOGRAM FOR DIFFERENT TYPES OF *Ss*

Data plotted for schizophrenia, from Hoagland; that plotted for children, from Lindsley; remaining data, from Kreezer, unpublished. Location of mean values for group indicated by rectangles. Symbol 'F.M.' stands for feeble-minded.

on the other. Some of the biological variables that have been examined are basal metabolism,[55] temperature,[56] sex,[57] and genetic factors.[58] A study of special interest was that of Davis on identical twins.[59] Marked similarities were found in the records

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Travis and A. Gottlober, How consistent are an individual's brain potentials from day to day? *Science*, 85, 1937, 223-224.

[55] D. B. Lindsley and B. B. Rubenstein, Relationship between brain potentials and some other physiological variables, *Proc. Soc. Exper. Biol. and Med.*, 35, 1937, 558-563.

[56] H. Hoagland, Temperature characteristics of the "Berger rhythm" in man, *Science*, 83, 1936, 84-85; Electrical brain waves and temperature, *ibid.*, 84, 1936, 139-140.

[57] H. Davis and P. A. Davis, Action potentials of the brain in normal persons and in normal states of cerebral activity, *Arch. Neurol. & Psychiat.*, 36, 1936, 1214-1224; G. Kreezer, unpublished data.

[58] H. Davis and P. A. Davis, *ibid.*; A. B. Gottlober, The inheritance of brain potential patterns, *J. Exper. Psych.*, 22, 1938, 193-200.

[59] H. Davis and P. A. Davis, *ibid.*

of members of a given pair of twins, suggesting the importance of congenital or hereditary factors in the EEG pattern of a given individual. In a study of the EEG in family groups, however, Gottlober did not find greater similarity within families than between different families.[60]

Among the psychological variables examined have been emotional excitability,[61] tendencies to introversion or extraversion[62] and tendencies to perseveration.[63] Although interesting relations have been reported, the results in this field may be regarded as in a very preliminary stage, with the question of the psychological and physiological correlates of individual differences in the EEG still largely an unanswered one.

*(c) Pathological variations.* Our third topic under the heading of psychological capacities concerns the variations which occur in pathologial states. Special fields here are the psychoses, mental deficiency, epilepsy, and pathological deviations of known organic origin.

Among the *psychoses,* fairly extensive surveys have been made of schizophrenic and manic-depressive patients.[64] Most investigators have failed to find significant differences between the EEGs of patients in either of these groups and mentally normal *Ss.* Lemere has, however, reported that manic-depressive patients tend to show 'good' alpha rhythms, and schizo-phrenic patients 'poor' alpha rhythms.[65] Statistical evidence for these conclusions is not given. The difference in alpha rhythm is attributed to differences in 'emotional lability' between the two types of *Ss,* the manic-depressive being regarded as exhibiting a high degree of emotional lability. In a study made upon the relation of emotional lability or excitability to the EEG in 110 feeble minded *Ss,*[66] no significant relation was found between properties of the alpha rhythm and degree of emotional excitability, except in the case of the Mongolian group. In this group there was evidence of an increased amplitude of alpha waves in the *Ss* of high excitability. The absence of a significant relationship in other mentally deficient groups, and for other properties of the alpha rhythm is not, however, necessarily incompatible with Lemere's conclusions, since a positive relationship might be present in psychotic *Ss* without its holding in *Ss* of a different type.

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[60] See foot-note 58.

[61] F. Lemere, The significance of individual differences in the Berger rhythm, *Brain,* 59, 1936, 366-375; G. Kreezer, A preliminary examination of the relationship between the electro-encephalogram and level of emotional excitability, *A.A.A.S.,* Dec. 1937, 174.

[62] A. B. Gottlober, The relation between brain potentials and personality, *J. Exper. Psych.,* 22, 1938, 69-74.

[63] L. E. Travis and J. R. Knott, Brain potential studies of perseveration: I. Perseveration time to light, *J. Psych.,* 3, 1937, 97-100.

[64] H. Berger, Über das Elektrenkephalogram des Menschen, XII, *Arch. f. Psychiat. u. Nervenkr.,* 106, 1937, 165-187; H. Hoagland, E. Cameron, and M. A. Rubin, The electro-encephalogram of schizophrenics during insulin treatments, *Amer. J. Psychiat.,* 94, 1937, 183-208; F. Lemere, The significance of individual differences in the Berger rhythm, *Brain,* 59, 1936, 366-375; L. E. Travis and W. Malamud, Brain potentials from normal subjects, stutterers, and schizophrenics, *Amer. J. Psychiat.,* 93, 1937, 929-936; P. A. Davis and H. Davis, The electroencephalograms of psychotic patients, *Amer. J. Psychiat.,* in press.

[65] L. Lemere, The significance of individual differences in the Berger rhythm, *Brain,* 59, 1936, 366-375.

[66] G. Kreezer, A preliminary examination of the relationship between the electro-encephalogram and level of emotional excitability, *A.A.A.S.,* Dec. 1937, 174.

An interesting series of studies upon schizophrenic *Ss* has been carried out by Hoagland.[67] He reports that in certain of the patients treated by insulin-shock therapy, a correspondence was found between the *delta-index* and clinical symptoms. The delta-index was larger when the patient became psychiatrically worse, and decreased when he improved. This measure was of value clinically only with patients with pre-treatment delta indexes greater than four.

In the field of *mental deficiency,* studies upon the relation of the EEG to intelligence level in the Mongolian and 'hereditary' types have been mentioned above. Exploratory investigations have in addition been made of other special types: hydrocephalus, microcephalus, phenyl-pyruvic amentia, and cretinism. The results point to differences in the EEG of different etiological or clinical types, and suggest that further pursuit of such investigations may help to throw light on cortical factors associated with mental deficiency.[68]

In the field of *epilepsy,* a number of striking results have been obtained: (1) the discovery of a special EEG pattern in petit mal seizures, a combination of a slow wave and a spike—recurring at a frequency of about three a second;[69] (2) the finding of this same type of pattern in *Ss* not diagnosed as epileptic, but who may show behavior-disorders of a sort frequently found among epileptics;[70] and (3) evidence that the physiological pre-conditions of a grand mal seizure may start developing a considerable time before the attack and may be detected in EEG long before any clinical signs occur.[71] The physiological interpretation of these results, and others, have thrown light on some of the mechanisms involved in the production of electric potentials in the brain.[72] The psychological implications of the results in epilepsy are not, however, altogether clear.

Studies have also been made of the EEG in a variety of pathological states *of known organic origin,* although in many instances the number of cases of a given type available was relatively small.[73] One result of psychological interest is the tendency for delta waves, of long duration, to appear in records of patients with brain lesions associated with marked psychological deterioration. It may be recalled that EEG patterns of this sort were also found during unconscious states, in mentally

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[67] Hoagland, Cameron, and Rubin, *op. cit., Amer. J. Psychiat.,* 94, 1937, 183-208.

[68] H. Berger, Über das Elektrenkephalogram des Menschen, III, *Arch. f. Psychiat. u. Nervenkr.,* 94, 1931, 16-61; Über das Elektrenkephalogram des Menschen, V, *ibid.,* 98, 1933, 232-251; G. Kreezer, Electrical phenomena of the brain among the feeble-minded, *Proc. Amer. Assoc. Mental Deficiency,* 42, 1937, 130-141.

[69] F. A. Gibbs, E. L. Gibbs, and W. G. Lennox, Cerebral dysrhythmias of epilepsy, *Arch. Neurol. & Psychiat.,* 39, 1938, 293-314.

[70] H. H. Jasper and I. C. Nichols, Electrical signs of cortical function in epilepsy and allied disorders, *Amer J. Psychiat.,* 94, 1938, 835-852.

[71] F. A. Gibbs, (Discussion), *Cold Spring Harbor Sympos. Quant. Biol.,* 4, 1936, 288.

[72] F. A. Gibbs, E. L. Gibbs, and W. G. Lennox, Cerebral dysrhythmias of epilepsy, *Arch. Neurol. & Psychiat.,* 39, 1938, 293-314; H. H. Jasper and I. C. Nichols, Electrical signs of cortical function in epilepsy and allied disorders, *Amer. J. Psychiat.,* 94, 1938, 835-852.

[73] H. Berger, Über das Elektrenkephalogram des Menschen, III, *Arch. f. Psychiat. u. Nervenkr.,* 94, 1931, 16-61; Über das Elektrenkephalogram des Menschen, V, *ibid.,* 98, 1933, 232-251; W. G. Walter, The location of cerebral tumours by electro-encephalography, *Lancet,* 232 (2), 1936, 305-308; C. L. Yeager and E. J. Baldes, The electroencephalogram in organic and nonorganic mental disorders, *Proc. Staff Meet. Mayo Clinic,* 12, 1937, 705-712.

normal *Ss*. The parallelism in the two sets of results may be of value in indicating factors in the brain on which normal psychological activities depend.

*Conclusion.* What estimate is it now possible to make of the probable value of the EEG as an indicator of cortical factors in psychological phenomena? It might be argued, on the one hand, that the manner in which the EEG is recorded makes it unlikely that it could be of value as such an indicator. The finding of rhythmical fluctuations of appreciable magnitude with electrodes attached to the scalp suggests that the records obtained depend on the activity of a large mass of cells and on the geometrical relations of these cells to the electrodes. It seems improbable that such organizations of electrical activity can correspond to the highly complex cortical organizations that are presumably associated with psychological phenomena, particularly since the latter organizations do not depend on the arbitrary relations of cells and electrodes which are involved in the recording of the EEG. A point of view such as this makes it seem unlikely that the EEG can provide an index of the specific cortical operations which correspond to psychological functioning.

On the other hand, even though the EEG is not likely to reflect the organization of the particular events in the cortex which underlie psychological performance, there are other ways in which it might serve as a psycho-physiological indicator, and thus contribute to an understanding of the cortical factors on which psychological phenomena depend. If, for example, certain properties of cortical cells play a rôle both in the production of the EEG and in certain psychological phenomena, then the EEG might conceivably be made to serve as an indicator of the nature of these properties.

It is not possible to determine, *a priori*, whether the EEG will, on the basis of this type of relationship, prove ultimately to be of value as a psycho-physiological indicator. Recourse must be had to explicit empirical steps, such as are represented in the diagram of Fig. 1. The first step in this program raises the question of whether correlations can be found between psychological variables and properties of the EEG. Our survey has revealed an appreciable number of such correlations, many of them tentative, between psychological and EEG variables. This set of results is compatible with the proposal to use the EEG as a psycho-physiological indicator, and justifies efforts to take the additional steps represented in the diagram. These results do not, however, guarantee the ultimate value of the EEG as such an indicator. They merely make its use in this way conceivable. For there are a variety of ways in which a given correlation between psychological and EEG variables might be explained. Not all of these modes of explanation would provide a satisfactory basis for the use of the EEG as a psycho-physiological indicator. With respect to the empirical correlations themselves, a great deal of work remains to be done. Not many of the investigations in this field can be regarded as more than exploratory. Improvement is necessary, particularly, in the procedures used for controlling and measuring the psychological variables involved.

The second step suggested by the schema of Fig. 1 calls for the determination of the particular cortical factors on which depend the EEG variables found to correlate with given psychological variables. The identification of these cortical factors will rest largely on the solution of the more general problem of the nature of the physiologial mechanisms responsible for various properties of the EEG. As our survey has shown, although physiologists have been actively investigating problems in this

field, scarcely any of the fundamental questions have as yet found final answers. It has been possible, consequently, for the second and third steps of our schema to be taken only by way of very tentative hypotheses. An example is Berger's hypothesis of the mechanism responsible for the depression of the alpha rhythm which follows initiation of different psychological activities. Further progress in this general field rests with further physiological research.

In conclusion, it seems safe to say that the finding of empirical correlations between psychological variables and EEG variables justifies further research directed toward the utilization of the EEG as a psycho-physiological indicator. The success of these efforts may depend in large measure on the ability of psychologists to formulate problems to which the EEG technique can effectively be applied, and on their skill in applying to these problems, in conjunction with the EEG, a variety of supplementary techniques.

Cornell University GEORGE KREEZER

## SPACE-TIME AS A PATTERN OF PSYCHO-ORGANISMAL MENTATION

The living organism has three essential and constituent elements: structure, function, mentation. It may be called a psycho-organismal individual. This is the postulate upon which the present discussion of space-time is written.[1]

Two of the elements of living things, structure and function, are organismal. Structure can be seen and described, although it is not fully known and is still the subject of scientific research. It is the field of anatomy, histology and embryology. It is spatial in quality. Function can be observed, and measured in physical and chemical terms. Time is essential to its nature. Mentation, on the other hand, at least in its higher levels, conforms to neither space nor time. It is an attribute of the psycho-organismal individual as opposed to the organismal. While it presumably cannot be alienated from the organismal elements of structure and function, it is neither. It is that which perceives structure and function, and constructs science and art out of them. Yet it is everywhere and always integrated with the organismal elements of space and time, and, like them, undergoes development.

In structure the living organism develops by expansion and individuation. Expansion is accomplished by incorporating into itself suitable substances, including the food material of the egg, and integrating them into an enlarging unit. But when this unit in unicellular organism reaches a certain size individuation intervenes to reduce the size by creating two individuals out of the one. Integration and individuation, therefore, are antagonistic principles of development. Integration tends to maintain the integrity of the individual; individuation tends to destroy it. The two principles are operating constantly throughout the life of the organism. In unicellular organism individuation is complete with every division of the cell, and two individuals arise from the parent by fission. In multicellular organism it is incomplete, and the daughter cells are integrated into a larger unit, and this is repeated throughout the history of the individual. But in many multicellular organisms individuation may be made complete by artificial means in early segmentation stages, and each cell may go on to form a completely integrated unit. Individuation should not be confused with differentiation. Both differentiation and de-differentiation can

[1] This investigation has been aided by a grant from the Josiah Macy, Jr. Foundation.

go on without individuation. It is as an antagonist of integration that individuation is most clearly recognized. Yet the two coöperate in all phases of development, and a normal individual depends upon a fine balance being maintained between them.

In the discussion of structure it has been necessary to employ physiological terms. This shows how intimately structure and function are associated. What an organism, a tissue or an organ does is its function. Function is structure in action. Structure is spatial; function is temporal. It is only in development that structure shows a temporal aspect, and then it is seen in the mirror of function—integration and individuation. So that what has been said of the development of structure applies equally to the development of function.

Mentation, it was observed, does not conform to space and time. It is true that mentation requires time, and is presumably always associated with organs that are spatial and that function in time. But memory, as a type of mentation, carries one backward in time and enables one to view the present as the future, and imagination defies both space and time and enables one to see the *there* as *here*, and the *then* as *now*. It is these and similar activities of mentation that exclude it from classification as one of the organismal functions. It is useless to try to force it into their mould. To do so only makes for confusion and error.

Presumably all will agree that, throughout the whole gamut of its existence, the individual, in structure and function, expresses itself through development; but some may find it difficult to appreciate the rôle of individuation in the development of behavior as the index of mentation. The difficulty arises largely from the use of the substantive *mind;* which, as an entity, must, according to our traditional mode of thinking, have being and existential origin. We have need, in this instance, as in the case of many concepts of science, of the Latin gerund. The classic Latins could think action independent of an actor, or a process as such apart from a substantive. By this means they gained clarity of thinking and avoided many semantic pitfalls; for substantives, in the growth of a language, may undergo change of meaning and attach to themselves subtle implication which become fixed by social heredity, and lead to error. Poincaré says that the use of the substantive *heat*, because it came to mean an indestructible substance, retarded physics for many generations.[2] Take *life*, for example. Apparently even some biologists hold to a concept of *life* as a specific entity which they hope to create, whereas it is the gerund *living* with which they are dealing—a process that is probably not the same in any two individuals, nor in the identical individual at any two successive moments; so that while the visionary scientist is creating *life* it becomes something different. So it is with *mind*. As a substantive *mind* stands for an entity, and as such it must have being and existential origin. Accordingly, many wonder whether or not animals have a mind, or when the human individual acquired a mind. On the other hand my conception is that it is a question of *mentation* not of *mind;* and mentation, like structure and function, began where and when the individual began, and, like them, undergoes development by expansion and individuation.

Exactly what constitutes mentation in the individual during the early phases of its embryonic development I do not know. Neither do I know what subconscious mentation is in the adult, but science and the medical art deal with it in a practical way. It is called the subconscious *mind*, and volumes have been written on the rela-

[2] H. Poincaré, *The Foundations of Science*, 1913, 281.

tion *it* holds to the conscious *mind*. The two types of mind are treated as separate entities which hold some definite and fixed relation to each other, with the result of endless confusion and contradiction. The concept of a psycho-organismal individual, with mentation as an element running parallel with structure and function, dispels the confusion and resolves the contradiction inherent in the theory of a subconscious mind underlying the conscious.

Mentation is uncritically regarded as the function solely of the cerebral cortex, and particularly of those parts which are known as sensory. This is probably due to the obvious connection of those parts with the sense-organs and sensory nerves, or to their relation to consciousness. But a very large sphere, if not all, of emotional mentation is associated with the thalamus, and there is no *a priori* reason why the motor parts of the brain and spinal cord, or the intercalary system of neurones, should not participate in mentation. Such an involvement of the motor system in sensory processes has been put by the eminent neurologist and psychiatrist, Adolf Meyer, in the form of a query: "Do we not have to get to the point where even in sensation, as in all mentation, we see a motor component, and do we not have to emerge from that medieval idea of having a 'purely sensory' sort of a world beside a 'motor' one?"[3] With the possibility of mentation of this order furnishing a clue to the origin and nature of space-time we may consider some of the relevant facts of the development of behavior as the index of mentation.

Movement, in the development of the individual, is primary. At least this is true of a wide range of vertebrates. That is, motility precedes sensitivity in embryonic development. This means that before there is a functional sensory system there is a functional motor system; and in certain animals the motor system, without a sensory, activates a complicated pattern of behavior which is carried over into later life and appears in the habits of the adult. This persistence of a pattern of action, while structures and functions are changing, is mnemonic. It is legitimate, therefore, to think of the motor system as having mentation, and of this mentation as being in terms of movement. But movement occurs neither in space alone nor in time alone. It represents both space and time, and, to the psycho-organismal individual, it *is* space-time. In the course of development space-time undergoes individuation into space and time insofar as the capacity of the individual permits; but that individuation is never absolute, as the confusion of language concerning these modalities proves.

Space-time is, then, a total pattern of mentation; and space and time are partial patterns arising within it. That it should be so is according to the law of development of behavior, not only in the lower realm of reflexes but also in the higher realm of human learning.

The development of reflex behavior is clearly seen in the salamander (Amblystoma). The first movement is in the anterior region—a turning of the head to one side. This bending, as the animal grows older, progressively involves more and more of the body, till it all moves as a total pattern of action. This is at a time when there are no motile appendages. When the appendages become motile they at first move only when the body moves, as a part of the total pattern of action. Only later do they have independent movement of their own so that when a stimulus is applied to them they will move of themselves while otherwise the animal lies quiet. This holds true for one appendage after another till all have acquired the ability of reflex

[3] Adolf Meyer, *Sensation: Its mechanisms and disturbances*, 1935, 271.

action; and the same principle maintains throughout the development of walking and feeding. With walking and feeding the ordinary definitive behavior pattern that is necessary for the animal's existence is completely established; and it has all come about through the expansion of an integrated total pattern and the individuation of partial patterns within it.

The application of this law of the development of behavior to higher mental processes and to learning has been made particularly by Ogden and Freeman in their *Psychology and Education*.[4] These authors hold that in the higher levels of learning, by the acquisition of knowledge as opposed to the acquisition of skill, four methods are employed: gradation, individuation, assimilation and re-definition. Gradation, although used by man in his thinking, is typical of lower animals, and need not be considered critically here. Assimilation is virtually synthesis, and as such means about the same as expansion of the total pattern, as I have used the term in the realm of reflex behavior. By re-definition equivocal situations are resolved, and their reintegration into new patterns in the transfer of learning is made possible. But it is the rôle of individuation, as understood by Ogden and Freeman, that has particular bearing on this discussion.

"These four ways of learning," they maintain, "can be found at different levels of behavior; for no single situation demands one kind to the exclusion of all the others. The processes of discernment here involved are not independent of each other, and one kind of transformation may involve or lead to another."[5] And they explain: "We do not mean to say that every process of individuation is an acquisition of knowledge, or even an act of learning. What we do mean is that all knowledge depends upon this process."[6]

That Ogden and Freeman mean by individuation the same as I mean by this term in treating of reflex behavior is clearly shown in the following passages: "Thus, the discovery of new bearings of one feature upon another within a total process of behavior, or the sudden emergence of a principle when one 'catches on' to a point or plan of action—these are instances of individuation at an advanced level of behavior, which may be acquired suddenly, and yet remain a permanently learned lesson."[7]

"The outcome of individuation as a means of acquiring knowledge, is, of course, the registration of the precise objects or things which can be used as tools and contemplated as units. The entire furniture of the world in which we live, including ourselves as the motivating agents and recipients of knowledge, is an outcome of this process of individuation which begins with the anatomical process of growth and development whereby the very organs of the body, which make knowledge possible, are individuated as partial-patterns of behavior." Again, they say: "every act of learning is a partial pattern of some larger whole from which it has been derived. Learning is never an arbitrary assemblage of parts."[8]

In these citations the higher levels of learning are brought into harmony not only with the principles of reflex behavior but even with growth and development of structure and function. In elaborating this idea the authors add: "The development of this assimilative process in which opposing forces are reconciled, leads not only to the resolution of conflicts, but also to the individuation of partial-pattern of behavior

[4] R. M. Ogden and F. S. Freeman, *Psychology and Education*, 1932, 1-350.
[5] *Op. cit.*, 261. [6] *Ibid.*, 247. [7] *Ibid.*, 252 f. [8] *Ibid.*, 300.

in which these several forces are dominant. In this process of individuation three distinct foci will appear. First, the focus of the original impulse will become the self-regulatory *want* of the organism for food, which, upon elaboration, become the discernible *Self*. Second, the barrier or obstacle with all that it implies becomes the discernible *means* of satisfying this want. And, third, the satisfying end, the food, becomes the discernible *object or goal* of the behavior as a whole. What the organism *heeds*, when it perceives its behavior, are these three foci: want, means, and end. These partial-patterns, when elaborated, constitute respectively, the self of contemplation and reflection, the techniques of living, and the world of things and ideas to be attained and avoided."[9]

With regard to volitional behavior Ogden and Freeman apply the principle in this way: "When we define the will as a behavior with insight dominated by a self-regulating desire, we are not defining an entity or agency that makes of the will a peculiar thing or event; we are simply pointing to the outcome of a development which began spontaneously, and perhaps aimlessly, and became directed and aimed, controlled and persistent, and finally intentional. This outcome is achieved typically by man, who possesses the capacity to individuate his behavior with respect to its varied aspects and thus to acquire a command over his own actions."[10]

In the field of social behavior these authors see the same principles at work, for they say: "It would, however, be wrong to suppose that sociability is merely added to the rest of the individual's traits—that an individual is what he is, even before his social traits are considered. On the contrary, individuality is a product of society; at least it is the product of some larger whole, the members of which must be individuated before they can become independent."[11]

In psychology, therefore, the concept of individuation as a psycho-biological or psycho-organismal process has been consistently applied to the general field of psychogenesis, and on that basis we are justified in applying it to space-time as a pattern of mentation. To derive it from the motor field of nervous action, before the sensory system is functional, carries it far back in psychogenetic history, before the dawn of the idea, of consciousness, or even of sensation. It may be regarded as having the nature of a general feeling, but more primitive and elementary—something subconscious that the organism brings into conscious existence with him. From it as a form of mentation all ideas concerning space and time have been individuated or abstracted; it is the matrix of all subconscious and conscious mentation.

Gainesville, Florida G. E. COGHILL

## DR. W. L. JENKINS ON THE VASCULAR THEORY OF WARMTH AND COLD

In a note in a recent number of this JOURNAL,[1] W. L. Jenkins critically examined Nafe's vascular theory of thermal sensitivity. In so short an article it is hardly fair to assume that all the criticisms recognized by him find expression, but rather that those he considers the more important are selected. He presents three series of comments: *(A)* those upon general statements and the indirect evidence offered for the theory; *(B)* the experimental evidence for the theory; and *(C)* the evidence against the theory.

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[9] *Ibid.*, 304 f. [10] *Ibid.*, 317. [11] *Ibid.*, 111.

[1] W. L. Jenkins, A critical examination of Nafe's theory of thermal sensitivity, this JOURNAL, 51, 1938, 424-429.

*(A)* Jenkins refers to the indirect evidence for the theory as "impressive in bulk," but he brings against it only four objections. *(1)* In regard to my attack upon the correlation between temperature sensitive points and receptors specific to thermal sensations, Jenkins says: "We may agree heartily with Nafe as to the inadequacy of these particular hypotheses, without accepting his vascular theory as the only alternative." With only two theories under discussion his point is not well taken. His remarks, however, hardly constitute a criticism of the theory.

*(2)* Jenkins says that "the histological findings which Nafe cites are meaningless." "To constitute direct evidence, one-to-one correspondence between the blood vessel clusters and areas of thermal sensitivity must be shown." The references in question include one to Goldscheider who conducted a series of experiments in which temperature sensitive points were carefully located and a needle thrust through the skin at the point in such a manner as to leave a stain.[2] The spot was then excised, fixed, and examined under the microscope. Goldscheider says: "Stets befindet sich die Ramification in auffallender unmittelbarer Nähe von Gefässen," which I translate as follows: "The ramifications (of the nerves of the temperature spots) *always* occur in *strikingly immediate* proximity to the (blood) vessels."[3] He further states that the structures are alike under both warmth and cold points, that they do not occur generally throughout the skin and specifically do not occur under pressure points. This fulfills Jenkins' requirements, the work has never been disproved or even questioned, and is one example of the citations which to Jenkins appear meaningless. It also should be noted that although Jenkins elected here to discuss "indirect evidence" and "general statements" his objection is that the evidence offered is not *direct* evidence. This tends to obscure the issues.

*(3)* Jenkins says further that "Nafe's tabulated 'correspondence' between smooth muscle activity and thermal experience at various temperatures is thoroughly misleading." The experience of 'heat' for example, he continues, is reported at temperatures much less than 45°C. and measurement beneath the surface shows that the temperature of the stimulus is not delivered beneath the surface of the skin. Inasmuch as the first of these points is made clear in the published articles and no one has ever, so far as I know, believed stimulus-temperatures to be delivered undiminished below the surface of the skin, there is nothing here to combat except the inference that I ever suggested other possibilities.[4] The essential point Jenkins entirely misses: the experiments show that at whatever temperature a sensation of heat is reported, the corresponding record shows vascular constriction; at whatever temperature warmth is reported the corresponding record shows dilation; and at whatever temperature cold is reported, the corresponding record again shows constriction.[5] Far from supporting the idea that the critical points for cold, warmth, and heat are constant in relation to temperature, it is one of the main points of vascular theory that temperature sensitivity is not constant in relation to temperature of stimulus but is constant in relation to vascular action.

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[2] J. P. Nafe, The pressure, pain and temperature senses, *Hdbk. Gen. Exper. Psychol.*, 1934, 1053 f.

[3] A. Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerven beim Menschen, *Arch. f. Anat. u. Physiol., Physiol. Abt., Suppl.-Bd.*, 5, 1886, 205 f.

[4] Nafe, *op. cit.*, 1049, 1054 ff.

[5] J. P. Nafe and K. S. Wagoner, The effect of adaptation upon vascular reactions to thermal stimuli, this JOURNAL, 49, 1937, 645-649.

*(4)* Jenkins objects because I stated that Bazett and McGlone, with puncture of an artery, reported "flashes of warmth and cold" whereas those authors actually said "sudden sensation of warmth." The point is not essential nor even important to vascular theory. Jenkins' concluding remark is that "no localized 'flashes' of warmth and cold are mentioned" but Jenkins himself is the only person to mention "*localized* flashes." I neither said nor inferred that they were localized but only the inference that I did gives any point to his paragraph.

*(B)* The experimental evidence for the theory is next considered in four sections. *(1)* The first discusses work upon the cornea of the eye which showed that the area insensitive to warmth and cold corresponds in general to the area in which there are no blood vessels but in which free nerve endings do occur. Jenkins presents no fact not brought out in the report of the experiment and does not criticize either method or procedure. The marginal sensitivity of the cornea is assigned by us to spread of effect outside the cornea. In the adjacent, outside areas are present both blood vessels and nerve terminations other than free nerve endings and, of particular interest in this connection, there are Krause endbulbs. The possibility of their functioning is not denied. Jenkins states that our conclusions are "non sequitur" but he gives no meaning to his verdict.

*(2)* The second section deals with the experiments upon a contralateral hand which showed a correlation between vascular action and the application of thermal stimuli. Jenkins suggests that these experiments do not necessarily prove the conclusions to be true and states that some "central irradiation process" is necessary to their explanation but he shows no defect in the experiment nor in the conclusions and no proof for his irradiation processes. Jenkins is presenting his own ideas but they do not constitute a criticism of the work under discussion.

*(3)* The next section has to do with an experiment upon the effect of stasis of circulation. Jenkins suggests that "Nafe's conclusions are based upon a comparatively small number of experiments. The coincidence of the failure of vaso-constriction and the loss of cold sensitivity may not be as precise as he has been led to believe. More research on this point is needed." The criticism is moderate and fair but inasmuch as similar limitations are suggested with the report of the work it seems a bit pointless. Such observations are made with extreme difficulty and involve great discomfort to the observer. Probably no more evidence of the kind will be offered by us but the showing, such as it is, remains unchallenged and squares with other work of the kind.

*(4)* The next study Jenkins states is less significant. It shows that a given temperature may, at different times, result either in constriction or dilation of the blood vessels, but in the former case is felt as cold and in the latter case is felt as warm.[6] He further states that the vascular action should occur whether or not it is related to thermal sensitivity. Of course it *does* occur. The problem may be less significant but if its significance were apparent to the critic it might prevent criticisms such as that in *(A, 3)*. The experiment shows that sensitivity and vascular action do correspond and of this there is no criticism offered.

*(C)* Jenkins concludes with seven sections of evidence against the vascular hypothesis. They are considered in the order in which they are presented. *(1)* Paradoxical warmth and cold are inexplainable in terms of the vascular theory. "To explain the phenomena purely in terms of the temperatures involved, it would be

[6] Nafe and Wagoner, *op. cit.*, 649.

necessary to assume that some blood vessels contract when stimulated with a high temperature. . . . Experimental evidence of such contradictory behavior seems to be lacking." The evidence isn't at all lacking. Reversals of action of visceral tissue are well known, reversals in the expected reaction of vascular tissue in particular, and some of it has been presented in the articles under discussion.[7] In general autonomic fibers exert only a regulatory effect over plain muscle rather than control comparable with the fibers of striate muscle. Plain muscle has inherent functions which proceed with or without innervation but which are normally modified by autonomic action, the action of hormones, the activity of the organ or tissue of which the plain muscle elements form a part, by their own rhythmical and tonic activity and by other factors. Under such circumstances it cannot within reason be expected that change in any one of these conditions will have a constant effect upon the activity of the muscle. Such effect will depend in part upon all the conditions present within the muscle at the moment. The conditions under which such reversals occur are becoming better understood but there is much remaining to be explained. Still less exactly do we understand the conditions under which paradoxical warmth and cold appear. Both this and the nervous effects are reversals of what is usually expected under the conditions of stimulation and to that extent there is a general correspondence between the two. On the other hand there are no facts involved in the explanation under the doctrine of specific end-organs and central areas, the specificity of the end-organ is just expanded once more, *in theory*.

*(2)* Certain chemical substances applied to the skin result in thermal sensations. "To maintain the vascular theory it will be necessary to show experimentally that cold-inducing substances cause vaso constriction consistently." My answer is that suggestions for a future experimental program is no part of criticism. To successfully criticise the vascular theory at present it is necessary to show experimentally that such chemicals do not have such an action.

*(3)* Jenkins says warm and cold sensations may result from mechanical and electrical stimulation and it is necessary for vascular theory to show that under the same conditions there is vascular constriction and dilation. This is an extension of the preceding criticism and the same answer applies. We have, however, made the showing for mechanical stimulation.[8] The opposed theory must show an end-organ for cold that is "specific" to cold, high temperature, electrical stimulation, mechanical and chemical stimulation. The concept seems to be bulging. Such end-organs are known only in theory, ours at least are known to exist.

*(4)* Jenkins cites evidence for areas of the skin which respond only to cold and points out that to account for this under the vascular hypothesis some blood vessels must be held to constrict with sufficient rapidity to arouse sensations of cold but fail to dilate in such a manner as to arouse sensations of warmth. This, he states, is not inconceivable but is highly improbable and experimental proof is lacking.

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[7] Albert Kuntz, The Autonomic Nervous System, 1934, 169-171 (for reversals of reflex action), 176 (for the effect of some hormones), 247 (for reversals in intestinal activity); C. J. Wiggers, *Physiology in Health and Disease*, 1937, 544 f. (for reversals in reflex action and the effect of certain chemicals), 549 f. (for reversals in heart action); E. H. Starling, *Principles of Human Physiology*, 1930, 197 (for reversals with mechanical stimulation), 198 (for reversals with temperature changes); W. H. Howell, *A Textbook of Physiology*, 1934, 927 (for discussion of reversals of action with adrenin).

[8] Nafe, *op. cit.*, 1056.

Without going into details it is freely admitted that there are areas which are relatively very sensitive to cold and relatively insensitive to warmth. There is no explanation. No one has made a study of the vascularization of these areas from the point of view of sensitivity. There have been unconvincing attempts to show a difference in the supply of afferent end-organs. This is the third suggested experiment, it is not a criticism of the vascular theory.

*(5)* Jenkins says "Adaptation phenomena remain a definite barrier to the ready acceptance of the vascular theory" but he doesn't say why. On the contrary we are about to publish data tending to show that the vascular theory has led to a very definite advance in our knowledge of adaptation.

*(6)* Jenkins cites Kastorf's showing that warm stimuli separated by "only a few tenths of a second" can be sensed as discrete and then states that the sluggish action of smooth muscle doesn't fit this fact. The answer is that the generally accepted whole reaction time for warmth is about five tenths of a second and so the time for the essential muscular response alone must be considerably less, perhaps "just a few tenths of a second." So until we have further facts we must believe that the fit of the two sets of data is very neat.

*(7)* Jenkins says that "thermal reaction-time seems to be another insurmountable barrier to the acceptance of the vascular theory," and he then proceeds to divide the whole time into four components: (1) time for penetration from the surface of the skin to the effective organ; (2) for vascular reaction; (3) for afferent nerve conduction; and (4) for central processes, efferent conduction and reaction. To these, unfortunately, he attempts to assign values without citing any evidence at all, "crude estimates" he calls them at first but he later states that "unless the computations can be shown to be grossly faulty, the vascular theory appears here to have encountered an unanswerable objection." Without any evidence of course no theory would suffer at all and his attempt to shift the burden of proof by challenging the supporters of vascular theory to disprove his figures is unjustifiable; it is entirely his problem to establish his own figures, in spite of which the following considerations are presented:

*(a)* Jenkins says: "Assuming the most superficial arterioles to be 200 μ below the surface and the rate of thermometric conductivity about 850 μ per second" it requires approximately 230 m.sec. for the stimulus-temperature to affect the essential organs." The rate of conductivity cited is satisfactory but he does not state where the measures of depth are taken, the thickness of the epidermis, or the area of the stimulus. One can later calculate from his figures that he has in mind a place about one-half meter from the brain although this figure is not given. If this can be taken as the trunk of the body, the thickness of the epidermis is about 77 μ.[9] The most superficial arterioles lie immediately under the epidermis and blood vessels also enter the papillae.[10] A value of 85 μ appears to be a reasonable estimate of the depth of the most superficial arterioles and with the rate of penetration cited by Jenkins would require 100 rather than 230 m.sec. for penetration to the essential organs.

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[9] B. J. Schneider, A study of the skin of the American whites and the American negroes. A thesis in the Department of Cytology, Washington University, St. Louis. Skin from chest and back of ten subjects showed an average thickness of the epidermis of 77 μ. If the epidermis of the forearm is included the average is 79 μ.

[10] Wiggers, *op. cit.*, 554; A. A. Maximow, *A Text Book of Histology*, 1934, 324.

*(b)* Jenkins allows, because of the slow reaction-time of gut, 100 m.sec. for constriction of the blood vessels. Roos, however, has shown that even the stomach muscles of a cold blooded animal (frog) may contract in 40 m.sec.[31] Reaction-times for constriction of vascular muscle in warm blooded animals are not available but it is known to be relatively very rapid at times. Roos has shown and Howell states that "the long latent period observed—under ordinary conditions—is obviously an artifact."[32] Beyond the matter of the latent time, as measured, however, it is well known that the greatest part of the latent period for muscles is used in building up tension against whatever resistence is involved, both in the muscle itself and in the measuring apparatus and it also is well known that the muscle is in process of change (contraction) for a relatively long time before a record of shortening is begun. Even contraction without shortening of the muscle (isometric) must be adequate to stimulate the afferent endings. If we assign a value of three-fourths that for the fastest reaction-time reported for gut in cold blooded animals it may be acceptable as a maximum figure: 30 m.sec.

*(c)* For afferent conduction over small visceral fibers Jenkins allows a rate of 5 m. per sec. making another 100 m.sec. (It is from these figures that we calculate the length of Jenkins' fiber to be ½ meter.) The afferents of the blood vessels are not all small. There are many heavily myelinated, "type B" fibers.[33] The speed of conduction over fibers depends upon the size of the fiber and on certain components of the myelin sheath, if any, and varies from about 0.5 to 120 m. per sec.[34] A conservative estimate of the speed of conduction for the fibers in question would be approximately 25 m. per sec. and for Jenkin's half meter this would require 20 m.sec.

*(d)* The time for the central processes, efferent conduction, and reaction, although given without any evidence at all, may be taken as a "crude estimate." This is 100 m.sec.

Jenkins' estimates of these times totals 530 m.sec. They are based upon no cited facts at all except for conduction rate of heat through the skin. Our estimates, based upon what facts there are available under the circumstances, totals 250 m.sec. which does approximate the best measures of reaction-time for cold. The reaction-time for warmth is about double that for cold which, under the vascular hypothesis, corresponds fairly well with the difference in time between relaxing and contracting muscle. Jenkins fails to mention the difficulty for the other theory.

Jenkins has, with his over-generous allowances of time, shut out the theory he is, in effect, defending. The vascular organs lie as high as, or higher than, any encapsulated end-organs of the dermis. Therefore, by his figures, the essential end-organs must lie at least 230 m.sec. distant, or further, from the surface of the skin. Without including afferent conduction time and latency, his own figures on reaction-time add another 100 m.sec., making a total of 330 m.sec., well above his stated

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[31] — Roos, *Arch. Néer. de physiol.*, 17, 1932, 57. We used the report in Heilbrum, *Outline of General Physiol.*, 1937, 414.

[32] Howell, *op. cit.*, 29.

[33] H. H. Woollard, The innervation of the blood vessels, *Heart*, 13, 1927, 319-336; P. Stöhr, Jr., Nerves of the blood vessels, etc. in W. Penfield's *Cytology and Cellular Pathology of the Nervous System*, 1, 1932, 383-419.

[34] F. O. Schmitt and R. S. Bear, The optical properties of vertebrate nerve axones as related to fiber size, *J. Cell. & Comp. Physiol.*, 9, 1937, 261-273.

basic reaction time of 300 m.sec., not to mention his preferred figure of 250 m.sec. or less. The calculation only demonstrates the futility of such guess work.

Jenkins' paper offers several sound suggestions for further experimental work but raises only two questions important to the work already done: (1) that of our explanation of paradoxical warmth and cold and (2) that of reaction-times. In both of these cases all the extant evidence, meager as it is, obviously supports the vascular hypothesis.

Washington University JOHN PAUL NAFE

## A BINOCULAR COLOR PHENOMENON

During recent years the investigations of such writers as Ferree, Rand, Graham, and others have made increasingly clear the importance of visual interaction for the problem of color theory. It is generally recognized, however, that, by reason of its implications relative to the existence or non-existence of special photoreceptor processes for the mediation of yellow and white, special importance attaches to the interaction process termed binocular color fusion. Since red and green fuse binocularly to give yellow, and since according to Sherrington there is very little convergence in the visual tracts leading to the higher nervous centers, binocular color fusion probably takes place at some brain level. That this level is higher than that at which color contrast occurs was shown by Hering's demonstration of the fact that the latter phenomenon takes place before binocular fusion. By reason of the importance of all instances of visual interaction for color theory the following case of color fusion is offered for consideration.

The Hecht Binocular Color Mixer, equipped with Wratten filters no. 58 (green) and no. 29 (red), was used to produce a 'red' image on the retina of one eye and a 'green' image on the corresponding retinal area of the other eye. Somewhat after Hecht's description, the left eye viewed a light through the green filter and a neighboring portion of white background through the red filter. The right eye viewed the light through the red filter and a neighboring portion of the background through the green filter. The result was a binocular yellow square flanked on the left by a green square and on the right by a red square; since the neighboring green in one eye and red in the other do not overlap they are seen monocularly as red and as green. The attempt to project the negative after-images of all three squares upon a background of white card board gave a violet square flanked on the left by a purple square and on the right by a blue-green square. The after-effects were highly unstable and of unequal latent periods. The violet square could be made to disappear instantly by closing or covering either eye after the three squares had been projected binocularly. The attempt to produce all three squares monocularly gave only the purple and the blue-green squares. It was found, however, that if shortly after securing the purple and the blue-green squares by means of monocular projection the eye which had been closed was opened, these squares seemed to "move over and make room for" the central violet square, thus giving all three squares simultaneously. As the writer became more practiced in making the projections the after-effects manifested a greater tendency towards the usual after-image sequence.

It appears that the above sensation of violet was either a negative after-image of the binocular yellow or a fusion product of the negative after-images of the red and green. Since we were unable to produce the same 'after-images' monocularly

it appears more likely that the violet was produced by binocular fusion of the negative after-images of the red and green. This case of fusion is probably similar in character to that which produced the binocular yellow and may be presumed to have the same general locus in the visual mechanism.

Vanderbilt University EUGENE G. BUGG

## William Stern: 1871 – 1938

William Stern was both a pioneer and a systematizer in psychology. His mind was one of the first to range several of the special fields that are now familiar territory. He will be remembered especially for his sure-footed explorations in differential psychology, forensic psychology, psychotechnics, child psychology, and intelligence testing. But he will be remembered likewise and, I think, with increasing renown for his theoretical system of personalistic psychology wherein he ordered his manifold research, and which, in turn, he incorporated within his comprehensive philosophic doctrine of Critical Personalism.

Stern's life—in spite of his last five troubled years of exile—was remarkable for its orderliness of purpose. Already at the age of nineteen he had planned the intellectual road that he would travel, and, never doubting, he pursued it to the day of his death.[1] His every act seemed an efficient expression of his dominant purpose. He worked eagerly, without lost motion, keeping throughout his lifetime a distinctive boyish zeal and optimism. His own enthusiasm made him an effective teacher, and his well-ordered life was reflected in the high morale of the two institutes that he directed.

Born on April 29, 1871, in Berlin, (Louis) William Stern lived in that city until he was twenty-five years of age. In his third semester in the University he began to study with Ebbinghaus, a fact which decided his training and his career. Yet the influence of Lazarus and of Paulsen, with whom he likewise studied, led him to an early decision that he must transcend the naturalistic views of Ebbinghaus and work out for himself a fuller and less limited conception of proper scope of psychological science. Even in his doctoral dissertation, *Die Analogie im volkstümlichen Denken* (1893), he attempted a synthesis of cultural and experimental science, blending, as it were, the teachings of Lazarus and of Ebbinghaus.

Before going to Breslau as Privat Dozent in 1897—at the invitation of Ebbinghaus who had moved there in 1894—Stern worked for five years upon his experimental monograph, *Psychologie der Veränderungsauffassung* (1898). In this work

[1] In 1926 when he had reached what he called a "provisional resting point" (after the appearance of the third and final volume of *Person und Sache*, his system of Critical Personalism) Stern wrote his intellectual autobiography which was published in both German and English (*Die Philosophie der Gegenwart in Selbstdarstellung*, ed. by Raymund Schmidt, 1927, 129-184; *A History of Psychology in Autobiography*, I, ed. by C. C. Murchison, 1930, 335-388). This source, though stressing Stern's philosophical work at the expense of his psychological, gives the best picture of Stern as man and thinker. Its bibliography, however, is by no means complete, listing only 36 selected titles up to 1926 (twelve years before his death). The *Psychological Register*, III, 1932, lists 95 titles for the same period; and 29 additional titles (1927-31). Since 1931, the *Psychological Abstracts* lists 15 titles. The total thus becomes 139 published articles and books including a few translations and basic revisions. Because of the availability of these bibliographical sources I shall not give full references to the works I cite.

we find an account of his invention, the *Tonvariator,* and likewise one of the earliest discussions of the 'conscious present.' Treating the perception of change in many sense modalities, the work foreshadows, though none too clearly, the purely phenomenological descriptions of the Gestalt school. It begins to break with the 'constancy hypothesis' and may fairly be ranked as an early expression of the *Strukturbegriff.* Yet, in this work the interpretation of the nature of structure is lacking. The author only vaguely felt the need for a substantial agent which he might designate as the cause and the carrier of perceptual change. The Person was not yet discovered. The logical completion of this early monograph was not reached until thirty-seven years later in his paper, *Raum und Zeit als personale Dimensionen* (1935).

Before his promotion in Breslau to the post of *ausserordentlicher Professor* in 1907, Stern manifested his originality to its fullest extent. The first seven years of this century were for him a period of true genius. In rapid succession there appeared his pioneering books and monographs in five virgin fields of psychology. (1) *Über psychologie der individuellen Differenzen* was published in 1900, and in it the author declared, fulsomely perhaps but none the less prophetically, that individuality would be the problem of the Twentieth Century. The book was completely rewritten in 1911 under its more familiar title, *Die differentielle Psychologie.* (2) *Zur Psychologie der Aussage* appeared in 1902, followed by two volumes of the *Beiträge zur Psychologie der Aussage.* It is interesting to note that his last public lecture delivered in New York City in December 1937 dealt with this same problem in relation to trial procedure at law. (3) The child studies likewise began at this time. Children's interest in their school work, their manner of reporting and the accuracy of their testimony first engaged his attention. There followed soon an account of the case of Helen Keller, and the two famous monographic studies based upon observation of his three children, written in collaboration with his wife, *Die Kindersprache* (1907) and *Errinerung, Aussage und Lüge in der ersten Kindheit* (1908). (4) Applied psychology was christened and presented to the psychological public in a brochure entitled *Angewandte Psychologie* (1903). In this publication he likewise advanced the concept of *psychotechnics* (eleven years before Münsterberg appropriated it and brought it to fame). In 1906 (with the help of his student Otto Lipmann) Stern founded the *Institut für angewandte Psychologie* in Berlin and shortly thereafter the *Zeitschrift für angewandte Psychologie* (1907). Stern remained co-director of the *Institut* until 1916 and co-editor of the journal until 1933. (5) In 1900 he began to work on the foundations of his Critical Personalism, choosing definitely in that year the title for his first volume and doing much of the writing. His first philosophical publication (1903) was an article considering the second law of thermodynamics in its significance for mental science, controverting the pessimistic deductions that Von Hartmann had drawn. To the same period belongs the first publication of his system, Volume 1 of *Person und Sache, Ableitung und Grundlehre* (1906).

Thus between the ages of 29 and 35, while still a Privat Dozent, Stern fashioned with marked sureness of touch new views and new methods in four important departments of psychology—differential, forensic, child, and applied; and at the same time accomplished the initial, and therefore most creative, portion of his theoretical work.

His work in child psychology, for which he is best known in America, soon grew beyond the bounds of the diary method. He became interested in more exact methods of study, and undertook to introduce into Germany the new movement of intelligence

testing. His influential book, *Die psychologischen Methoden der Intelligenzprüfung und deren Anwendung an Schulkindern* (1912) was twice later rewritten. It was in this work that the concept and first formulation of the I.Q. were advanced. Soon followed another notable book, written from the normative and developmental point of view, *Psychologie der frühen Kindheit bis zum sechsten Lebensjahre* (1914). This volume found its way into several editions both in German and in English.

In 1915 Ernst Meumann died in Hamburg. Stern immediately became his successor as editor of the *Zeitschrift für pädagogische Psychologie*, and in 1916 his successor as professor of philosophy, psychology and pedagogy at the *Kolonialinstitut* and director of the psychological laboratory that Meumann had founded. After the war the demand for new universities, caused by the return of the soldiers, led to the transformation of the colonial institute into a full-fledged university. In this transformation Stern himself played a strategic part by first inaugurating informal and private courses for the returned soldiers.

At Hamburg the interest in child psychology spread in a practical direction, partly to satisfy the demands of the local school system. Valuable researches are to be found in the *Hamburger Arbeiten zur Begabungsforschung*. The Institute soon became a well-known center for research in educational and vocational psychology. All the while fruitful researches in psychotechnics, legal psychology and experimental psychology were continuing. In 1925-1928 Stern gave considerable attention to the study of puberty and adolescence, planning to extend his treatment of child psychology through these ages. Several miscellaneous papers mark this period in addition to the monograph, *Anfänge der Reifezeit* (1925), containing a psychological analysis and commentary on the diaries of a boy from his twelfth to his fifteenth year. This work takes on special interest when the reader comes to realize that the anonymous writer of the diaries and the commentator are one and the same person. Stern as boy and Stern as man were too much alike to make complete disguise possible!

The story of the Hamburg Institute (1916-1933) is told by Stern himself in four reports.[2] The last is an ominous *Schlussbericht* (1933), a hastily written, but as ever orderly and workmanlike report, winding up the affairs of an active, democratic, and renowned center of psychological research.

The story of his last five years serves best of all to reveal his character. His first asylum was in Holland where he worked doggedly on his last and most comprehensive volume, *Allgemeine Psychologie auf personalistischer Grundlage*. Unable to issue it in Germany, he found for it a Dutch publisher (1935). With this writing completed, he joined the trek of exiled German scholars to America, accepting a position as lecturer and later as professor at Duke University (1934-1938). Though he had visited America twice before (once for the celebrated conference at Clark in 1909, and a second time for the IXth International Congress at New Haven in 1929), he spoke little English and was entirely unfamiliar with American educational practices. Courageously, he and his gifted wife, who was his close collaborator since their marriage in 1899, set to work to master a new language and a new way of life. Besides teaching at Duke he gave occasional lectures in Eastern colleges and

[2] Das Psychologische Laboratorium der Hamburgischen Universität, *Zsch. f. pād. Psychol.*, 23, 1922, 161-196; Aus dreijähriger Arbeit des Hamburger psychologischen Laboratoriums, *ibid.*, 26, 1925, 289-307; Das psychologische Institut der Hamburgischen Universität in seiner gegenwärtigen Gestalt, *Zsch. f. ang. Psychol.*, 39, 1931, 181-227; Aus den letzten Arbeiten des psychologischen Instituts der Hamburgischen Universität, *ibid.*, 45, 1933, 397-418.

taught in the Harvard summer school in 1936. He held two honorary degrees from American educational institutions (Clark, 1909 and Wittenberg, 1928). America had been good to him, and so with bright prospects here he undertook the task of refashioning his personal and professional life. His chief desire was to introduce personalistic psychology into America, to counteract, as he said, the "pernicious" influence of his earlier invention, the I.Q. In February 1938 he was deeply gratified by the appearance in America of the translation of his comprehensive text, *General Psychology from the Personalistic Standpoint*. For his next work he planned a new volume on child psychology. Suddenly, in the night of March 27, his work was ended, his prospects over. He died, without warning, of coronary occlusion.

Stern had the completest confidence in psychology, never doubting its firm position among the sciences nor its practical value for mankind. He had equal confidence in his own mission as a leader in theoretical and applied work. Consistent with this self-confidence were his ease and dignity of manner and his fluency and lucidity of expression. These qualities, combined with the vivacity and enthusiasm of which I have spoken, gave him a prominent place in public gatherings. He was a gifted conciliator. When tempers were frayed in debate, when awkward pauses followed an awkward scene, or when the occasion called for friendly remarks, it was he who could be relied upon for the right word easily spoken. Though fully conscious of his prerogatives as a German professor, and vocal about the merits of his own work, he was at the same time responsive to the thought of others and patient and kind in all his personal relations.

Elsewhere I have written an exposition and critique of the personalistic system as set forth in his final book.[1] In this brief memorial sketch I shall not enter again into the subject. It should, however, be pointed out that, unlike many philosopher-psychologists, Stern held to the end of his life his interest in the detailed problems of general psychology. His philosophical work may be said to have been completed with the publication of his *Wertlehre* (1924) or perhaps with the short volume, *Studien zur Personwissenschaft* (1930). In his last years he returned (with only a slightly relaxed grasp) to the finer details of general psychological analysis. His final text covers much the same ground as any modern text. The difference, however, lies in its tireless locating of every fragment and fact within its proper personalistic niche. For him all difficulties were resolved in the Person. This concept, and this concept alone, he thought, could provide the substantiality, the causality, and the individuality required in every mature psychological analysis.

It troubled him relatively little that his formulations ran counter to the trend of the times, particularly in American thought. Intellectually he lived largely in a world of his own making, and he believed so intensely in the liberating powers of personalistic thought that he had faith in its ultimate acceptability to others. He simply could not believe that psychologists would dwell for long within the narrow cells that they had created for themselves by a prior act of freedom. Any system other than personalistic, he felt, makes a travesty of the very mind that created it. Thinking in this vein Stern became a monumental defender of an unpopular cause.—And what of that? he would ask. Are there not fashions and fashions in psychological theory? The personalistic way of thought will yet have its day, and its day will be long and bright.

Harvard University GORDON W. ALLPORT

[1] The Personalistic Psychology of William Stern, *Char. & Pers.*, 5, 1937, 231-246.

# BOOK REVIEWS

*Einführung in die Völkerpsychologie.* By WILLY HELLPACH. Stuttgart, Ferdinand Enke, 1938. Pp. v, 178.

Admitting that no adequate definition of a people (Volk) exists, Dr. Hellpach proceeds from the empirical fact that peoples are the most persistent forms of communal existence, and notes that even the simplest group of this kind exhibits the primitive phenomena of (1) leadership and (2) a twofold division of relationships and regulations into (a) those determined by common blood and (b) those based on communal association. Here so to speak are the biological coördinates of the subject.

The first section of the work deals with peoples as natural phenomena which represents a distinct advance in relating the life of peoples to a more general biological background. The author is thus enabled to restore validity to the conception of communal organisms as contrasted with that of communal organizations. With this general principle as a guide, Hellpach offers interesting interpretations of the vitality, decline, death, rejuvenation, and self-transformation of peoples. External forces effecting transformation include changes in natural selection, immigration in various forms, and variations in custom, morals, sentiments and theory. Another important factor is increase or decrease in longevity and the proportions of individuals of different age. Racial ethnomorphoses are likewise of profound influence. A people may consist of a mixture or merely of an aggregate of races. In the former case intra-racial or intra-individual tensions, in the latter inter-racial and inter-individual tensions arise and create problems and tendencies unfamiliar to racially integrated peoples. Finally geopsychic factors such as climate, weather, soil and landscape, set limits to migration and leave their imprint on the constitution of the individual and the lines of social structure. In concluding this first major section it may be stated that Dr. Hellpach's emphasis on basic factors, should prove a useful corrective to the one-sided emphasis on derivative phenomena which seems customary in the study of folk psychology.

In the second section, Dr. Hellpach discusses the primitive possessions, institutions, and mental laws of evolution of the group. Primitive possessions of early people include language, costume and ornament, tools, law, and a conception of another world. With brief suggestions of possible origins, the author, formulates laws of change involved in the development of these primitive endowments. Linguistic changes depend upon three pairs of forces: (a) chance and the arbitrary decision of groups or individuals; (b) the inherent logic of language and linguistic creation; and (c) evolution and law. More minute changes depend upon the linguistic reflection of local group peculiarities in accordance with the law of ideo-motor response.

With a brief reference to the vast differences separating races and peoples in the invention and use of tools, the author traces the origin of work to necessity and the play impulse, and formulates laws determining the evolution of industry and property. Dr. Hellpach discusses clothing and ornament but briefly, ascribing their origin to magical factors. Laws and institutions also receive but brief treatment, the author stressing the universal tendency toward rigid static forms, which in turn create popular forces making for dissolution through such phenomena as revolutions,

reformations, romantic movements and renaissances, all representing new directions of popular motivation. The duration of laws and institutions is proportional to the degree to which they are organic configurations of a people as a whole.

The conception of another world leads to myth, magic, and religion, culminating in individual ethics and political organization. Totemism, theocracy, the ancient polity, political rationalism, and the almost untranslatable "Volkstum," constitute the successive stages of social existence as conditioned by the magico-religious motive in its successive transformations. From a certain standpoint, all might be viewed as transformations and elaborations of primeval myth which in turn derives from a still more primitive sense of mysterious forces and entities.

Dr. Hellpach's discussion of stages of folk existence is suggestive, and his interpretation of "Volkstum"—which may be best understood as the peculiar state of popular consciousness, which is retrospective as well as prospective, creative rather than aggressive, and concerned with a kind of esthetic goal—is illuminating.

In an interesting subsection Hellpach summarizes and contrasts the cultural philosophies of leading sociologists and historians of culture, noting the antitheses presented in their various treatments of social evolution.

The final section of this work is concerned with peoples as the creation of will, pointing out the popular sources of collective creativeness in the spirit of Herder, but in the light of modern knowledge of social inheritance, distribution of population, and the rôle of invention and leadership. The author stresses the distinction between civilization and the broader concept of culture which represents the determination of every aspect of a people's life by its highest value. The history of a culture is indeed the conscious configuration of communal life under the direction of creative volition.

In many respects Dr. Hellpach's work is an advance on earlier labors in this field. He offers a configurative psychology of the ethnic group, written according to the principle of genetic method and with a just sense of the proportions and limits of his subject. Like Wundt he is inclined to emphasize linguistic psychology, but not to the extent of making it the central core of folk psychology. He states laws wherever possible but is thoroughly aware of the relativity of generalizations and calls attention to many unsolved or possible problems in the field. He is somewhat impatient of academic quibbles concerning definitions, *e.g.* of race or folk, preferring to deal with existent functional realities as such. He is indifferent as to the technical status of folk psychology—whether it is psychology, sociology, a medley of both, a discipline, a method, or a mere point of view. If folk psychology yields new scientific truths, it is justified. Perhaps this is a sensible pragmatic criterion of any science.

University of Cincinnati CHARLES M. DISERENS

*General Psychology from the Personalistic Standpoint.* By WILLIAM STERN. Translated from the German by Howard Davis Spoerl. New York, Macmillan Co., 1938. Pp. xxii, 589.

Stern's *General Psychology* is important as the final organization of his psychological thought and also as a well-integrated view of the field of psychology. For these reasons it is fortunate that Dr. Spoerl completed this authoritative translation before Professor Stern's death. The "personalistic standpoint" is a broad, general framework upon which the facts are exhibited, and it gives these facts new significance. In his preface, Stern speaks of a "new psychological doctrine." It is not a

doctrine in the sense of something which calls for faith or blind acceptance, but rather a broad outlook upon the facts.

The German edition of the book has already been reviewed in this JOURNAL (48, 1936, 714-715). It is not, therefore, our province to reconsider the contents of the work. Before considering Spoerl's translation, however, we should like to make a remark about the personalistic point of view. A resemblance between Calkins' 'self-psychology' and Stern's personalistic approach was mentioned in the earlier review. There is, however, one important difference.

In Stern's words, "the 'person' is a living whole, unique, striving toward goals, self-contained and yet open to the world around him; he is capable of having experience." The active, functional character of the concept of 'person' is borne out by the greater part of Stern's presentation. Moreover, the person is a unity which carries on in this active way the life-functions as well as the functions called loosely "having experience." Calkins' *self*, on the other hand, is more passive—it is that which *is* conscious—and it is 'mental,' having to do only with experience and consciousness. *Person* and *self* resemble one another in that both may be (in Calkins' terms) "subject and object," the experiencer and the experienced, but the mentalistic flavor of the *self* is much less prominent in the concept of person.

A detailed analysis of Stern's point of view is, of course, impossible in a brief review, so we must be content with this comparison. Whether the reason is to be found in the nature of Stern's fundamental concepts, or in the manner in which the facts are integrated under these concepts, the final result is that the *General Psychology* takes account in a sensible way of a great deal of modern thought and research. The point of view contributes new significance and better integration to a great many topics.

The translator has succeeded in producing a remarkably faithful rendition into English. Occasionally a freer translation would have served better the interests of style, but the consequent dangers of misunderstanding and distortion are very great.

Although this volume is smaller than the German edition, the reduction is due chiefly to a change in type-face. Stern remarks in the preface that "considerable changes have been made" but actually only a few brief sections (largely repetitious) have been omitted, and some new material has been added. Included in the new material is a section on tests of imagination. Illustrative materials have been added here and there throughout.

The major alteration has been Stern's revision of the bibliography. The original list (the work of Dr. Betty Katzenstein) has been shortened, new English titles have been added, and works in translation are listed by their English titles. As a sample of the change the reviewer has made a rough count of one section of the bibliography. Out of 225 titles originally in the section, 111 (almost all German) have been dropped, and 33 new titles (almost all in English) have been added. In addition, 22 of the original German titles are now listed in English translation.

Spoerl has performed a service in making this important work available to the student whose knowledge of German is limited. While the book is too advanced for an elementary course, it will serve as an excellent text for advanced courses in general psychology or for reference in courses on special topics.

Cornell University T. A. RYAN

*Personality: A Psychological Interpretation.* By GORDON W. ALLPORT. New York, Henry Holt & Co., 1937. Pp. xiv, 588.

In a field which has too frequently been characterized by loose thinking, faulty definition, and narrow interpretation, Allport has succeeded in producing a precise, well-integrated, and thoughtful review. He has also interwoven into the pages of the book a coherent defense of the individual as a proper subject for scientific study. The result is a volume which is certain to exert a marked influence in future researches and interpretations in this field.

Adopting as his thesis the conception that nomothetic studies of 'the typical man' hide most of the important aspects of personality, Allport consistently proposes an attack on the problem by studying individual men. By way of a healthy counterblast to those who attempt to interpret personality entirely in terms of one's effect on others, he insists throughout the volume that the primary interest of psychology "is not in the factors shaping personality, rather in personality *itself* as a developing structure." With this background he proposes to make a comprehensive survey of the existing literature and "to supply new coördinating concepts and theories where they will equip this new department of psychology for a more adequate handling of its endlessly rich subject-matter."

The volume begins with a section which reviews various definitions of personality and surveys the varied approaches that have been made to a study of this topic. The second large section, entitled "The Development of Personality," deals with the fundamental influences which affect the growth of individuality (including such aspects as differentiation, integration, maturation and self-consciousness) and presents the author's theory of functional autonomy. "The dynamic psychology proposed here regards adult motives as infinitely varied, and as self-sustaining, *contemporary* systems, growing out of antecedent systems, but functionally independent of them." The theory is clearly presented and well supported.

The third major section has to do with "The Structure of Personality." It begins with a discussion of equivalence of stimuli and involves an excellent and detailed critique of the conception of specific, identical elements and of transfer theory. It continues with a review of the theory of traits and of the unity of personality. "Traits are not creations in the mind of the observer, nor are they verbal fictions; they are here accepted as biophysical facts, actual psychophysical dispositions related—though no one yet knows how—to persistent neural systems of stress and determination."

A section on "The Analysis of Personality" is concerned with the techniques that have been proposed for the study of personality. In all the author names more than fifty such methods; he insists that "there is no 'one and only' method for the study of personality." The major methods are subjected to critical review. The volume concludes with a section on "Understanding Personality," which reviews the evidence on inference and intuition and includes a section dealing with Personalistic Psychology—an approach which Allport refuses to espouse, albeit he evinces sympathy of it.

Allport is at his best when weaving together disparate threads which jointly constitute the web of proof or of criticism of some theory. He is at his worst when supplying catalogs of techniques or of meanings. Mere length of a catalog seems to impress him, with the result that little care is taken to preserve homogeneity in

the categories of his classifications. He has unusual ability to view the contributions of others *de novo*. Where others for years have accepted certain studies as 'settling' certain problems, Allport casts a critical eye at their shortcomings and demonstrates that they have contributed little or nothing to any final solution.

The volume is consistently dialectic in tone. If there are researches which fit the trend of the argument, well and good; if researches are lacking, the lack is made to seem unimportant to the smooth flow of logic. If certain methods of observation have marked advantages as regards controls and checks in comparison with certain other methods, there is nothing in this volume to suggest that. Any thorough-going positivist will find much to pain him in a careful reading of this text; and the general reader may justifiably resent the air of militant certainty which leads Allport to reject as 'misguided' certain researches with whose postulates he disagrees.

The faults of the volume are, however, easily forgiven in the light of its positive contribution. It is not an attempt to capitalize on popular interest by a ready acceptance of popular terminology; it is a catholic, coherent, and thoughtful review of a topic that has suffered overmuch from a lack of those qualities. A special debt of gratitude is due the author for his refusal to be stampeded by any of the currently popular techniques of approach. He has succeeded in presenting a thesis which cannot be ignored by those who are at work in this general field.

University of Maryland JOHN G. JENKINS

*Integration: Its Meaning and Application.* By L. THOMAS HOPKINS and others. New York, D. Appleton-Century Co., 1937. Pp. xiii, 315.

This volume, a manifesto of Progressive Education, has fourteen chapters, two appendices, and an index. Seven of the chapters are independent essays on "Integration as an Educational Concept" (Lindeman), "The Problem of Integration" (Wheeler), "Philosophic Aspects of Integration" (Harris), "The Biological Basis of Integration" (Buchanan), "The Psychological Evidence for Integration" (Watson), "The Psychiatrist Views Integration" (Mackinnon), and "The Arts and Integration" (Browne). The first chapter, "Viewpoint," and the Chapters IX–XIV on "Conditions Influencing Integrative Behavior," "The Correlated Curriculum," "The Broad-Fields Curriculum," "The Core Curriculum," "The Experience Curriculum," and "Integration Affects Courses of Study," are by the editor, two of them in coöperation with I. A. Hammer.

There is very little relation among the first seven chapters, and the remainder of the book by the editor is a justification of the "experience curriculum" in contrast with other types of educational method and practice. The term integration, which gives title to the book, is described by the editor in so broad a manner that its meaning is not different from that of education. Nor is any effort made to correlate the varied use of the term by the coöperating writers. With the possible exception of the chapters by Wheeler and Watson, the entire book exemplifies what Stuart Chase has called "the tyranny of words" which have no definite reference. For instance, "Personality and character are very closely related. The former describes the differentiated tendencies to behavior, whereas the latter evaluates these tendencies in the light of standards developed in and accepted by the particular culture. In other words, character is personality socially evaluated. Personality is developed in and affected by culture, but character is even more engrained in it." The definition refers to *behavior, standards* and *culture,* all of which lack definite meanings, and are what

Stuart Chase calls "blabs." Nor do "differentiated," "tendencies," "evaluates," "accepted," and "engrained" throw any light upon the relations and references of the terms *personality* and *character* which are being defined.

It is painful to think that a movement which calls itself progressive, and which has done so much to challenge the wooden methods of routine teaching, should be at a loss to express its philosophy in simple one-valued meanings which can be applied to its methods and practices.

Cornell University R. M. OGDEN

*Creative Group Education.* By S. R. SLAVSON. New York, Association Press, 1937. Pp. vii, 247.

This book is a strange mixture of social psychology, individual differences, mental hygiene, rejected views of original nature, vivid practical experience, instances of patient and obviously successful "parental" guidance and much wishful thinking. Slavson's groups are living realities quite untouched by any experimental analysis and if his methods lead to creative effort, it is by the sheer genius of a leader and not because group discussion is shown to possess hitherto unknown powers. By inference, the careful study of growth is as irrelevant to the socialized citizen of a democracy as is the formal school program even when it is based upon experimental work of the most recent vintage. Yet this is not to be taken as reproof, for the intimate friend of young people who is trying to meet them in every phase of their fast-moving attitudes and desires simply cannot talk the language of the laboratory or of the controlled schoolroom. One may well wonder, however, whether one man's testimony or the testimonies of many persons, even though they be ever so good, supply the means of sure progress in educational method.

The text for the book is as follows: "All important education is derived from group experience" (p. 10). What is important, of course, is the development of the "total personality;" and the "group experience" is not so much a formal student-teacher relationship nor formal education at all as it is group participation in what many teachers would call extracurricular activities. In more detail, "The four main contributions of the good family, and of all group education, are: (1) to establish satisfying affective (love) relations with children and with adults; (2) to provide ego satisfactions; (3) to give expression to the creative-dynamic drives of the individual; and (4) to engender emotions and to establish attitudes that dispose the individual to social usefulness and group participation. These are at once the objectives and the criteria for evaluating good group work" (p. 16), and they are essential "if we desire to preserve democracy and save civilization" (p. 5).

One method for realizing the above objectives is discussion (Chapter V) under able but not obtrusive leadership (Chapter II). The treatment of leadership is as good, perhaps, as most—which is only to say that here is a field of research much in need of further examination. If the author's notes are to be of service to others they must be put in a form which will make them common to all situations and not differential to the author's own rapport with students. The same holds true of discussion. Socrates long ago made it plain that discussion may be "creative" but it may also be trivial and inconsequential. It is not discussion itself which gains the results, but discussion *of* something. The "something" may be gained, as the author says, from group experience but it is also gained in many other ways, many of which are neither easy nor quite so casual as the progressive movement would like to believe. Any

group activity may be called creative if the word "creative" is confused with "self-expression."

The difficulty faced by the author on this score is shown by the relation of the so-called creative subjects to education. There have been, of course, creative artists in the various crafts (Chapter VI), in art (Chapter VII), in music and the dance (Chapter VIII), and in dramatics (Chapters IX, X). It does not follow, however, that exposure to the results of artistic creation or a discussion of them, or even self-expression regarding them are, in and of themselves, equal to creative ability. It may be questioned whether any real "creator" would agree that "training, such as an apprentice is subjected to, does increase skill and efficiency of production, but it does not tap the resources that true education releases" (p. 89). If a group is to have something to talk about it must assuredly know the arts, make contact with nature through science (Chapter XI), take frequent trips and excursions (Chapter XII), attend parties (Chapter XIII), do more than idle away all holidays (Chapter XIV), and excel its previous achievements in a gymnasium (Chapter XV), but mere exposure is not learning. The faults of the formal school system are no doubt legion but the remedy does not lie in a total neglect of studies on original nature, on processes of learning which, in practice, must often be laborious, or on any other phase of experimental psychology including the very difficult concept of creative activity.

Here again no reproof is intended. If the manners, ways of thinking and intimate attitudes of "good family life" can be brought into the school and into affiliated activities, it should be done. The author of this book appears to have done it. His readers will, no doubt, be grateful to share his experience. They will be excited by glimpses of some of the intimacies of human fellowship; but excitement is one thing and the sharing of well-attested facts about human growth is quite another. Even this sort of sharing can be "creative."

University of Illinois COLEMAN R. GRIFFITH

*The Movies on Trial.* By WILLIAM J. PERLMAN. New York, Macmillan Co., 1936. Pp. xi, 254.

Perlman presents the volume as a crystalized view of various opinions of outstanding personalities of the stage, screen, pulpit, press, bench, and classroom regarding the recent controversy with respect to the influence of the movies upon American life. Among the nineteen contributors are to be found such names as William Allen White, Raymond J. Cannon, Edward G. Robinson, Ben B. Lindsey, Brock Pemberton, Upton Sinclair, Sidney E. Goldstein and the Most Reverend John I. Cantwell.

Some of the contributors put the movies down as positively indecent, while others seem not to be so much disturbed about the low moral tone of the movies as they are about their vapid and inane character. The movies, however, have their defenders. Lindsey speaks more feelingly for them, and W. E. Blatz closes his chapter thus: "Until some one devises a more pleasing, a more efficient and more thrilling technique for passing on these delights than the movies, they will remain with us and will influence us for good or ill depending largely how we have been trained to interpret them."

The contributors do not look to censorship as a means of correcting the evils that exist. Box office receipts speak the most emphatically. The producers are urged by the contributors to develop the attitude of the creative artist, to make some appeal

some of the time to the more intelligent public. The home, the church, and the school are advised to cultivate in youth a discriminative attitude and to give more attention to their own intrinsic appeal.

The book is very readable and interesting but the reviewer doubts that it will change the opinion of the readers. The reader is sure to find somewhere in the book rather ardent support of whatever opinion he may have had. If he was for the movies, he will still be with them; if he was against them, he will leave the book a little more sure of his opinion. In only one of the chapters is there any scientific evidence in support of the opinions that have been given. The tone is inspirational rather than informational.

Iowa State College THOMAS F. VANCE

*An Introduction to Symbolic Logic.* By SUSANNE K. LANGER. Boston and New York, Houghton Mifflin Co., 1937. Pp. 369.

At a time when psychologists become increasingly aware of the value of strictly defined concepts and a rigorous technique of deduction a book like this should be very welcome. Formal exactitude and explicitness are no longer the prerogatives of mathematics and physics. The clearest and most definite way of stating a scientific theory is its formulation in terms of a set of independent postulates. This procedure has the advantage that the relation of facts (data) to the theoretical assertions become so evident that in the case of a modification of the theory one sees clearly which postulates are affected. Although Mrs. Langer does not make direct application to the empirical sciences she provides the basic logical tools necessary for the construction of any deductive system. She thus presents an admirable introduction to this relatively new field, written in her lively, lucid manner and replete with well chosen examples and applications. There are enlightening remarks on the nature of logical form, on abstraction and generalization, on language and symbolism.

Among the technical subjects treated are the quantification of propositions, the logic of classes and relations, postulate systems in general and the postulate system of the class calculus as well as the elements of the calculus of propositions. A special chapter deals with the (generalized) algebra of logic (Boolean algebra). Throughout the book Mrs. Langer never forgets to explain the meaning and purpose of whatever special technique she may deal with. It is interesting and significant to see that the Aristotelian doctrine of the syllogism—taught for more than two thousand years as the main part of logic—could be taken care of in a brief appendix and as a small part of the class calculus.

The book serves well as an introduction to *Principia Mathematica* of Whitehead and Russell—the classical treatise of mathematical logic and the logic of mathematics. It might be of even greater value if the logic of relations had been included to a fuller extent. Applications in this field are of great interest especially to the scientist.

University of Iowa HERBERT FEIGL

*Manual of Psychiatry and Mental Hygiene.* By AARON J. ROSANOFF. 7th edition. New York, John Wiley & Sons, 1938. Pp. xvi, 1091.

This book constitutes an almost complete rewriting and enlargement of the 1927 edition. It incorporates all of the major advances made in psychiatry and related sciences since that time: such as the new data on heredity, especially as regards

mental diseases in twins, new findings regarding cerebral birth injuries, infectious encephalitides, avitaminosis and other nutritive disturbances, Pavloff's researches especially of induced neuroses, new studies of delinquency and crime, the insulin treatment of schizophrenia.

Part I is devoted to "General Psychiatry," etiology, symptomatology, neuropsychiatric syndromes, and neuropathology. The chapter on syndromes deals with groups of symptoms which tend to occur together and which may appear in quite different mental diseases. This list of syndromes is quite extensive, covering familiar types (stupor, delerium, chorea, epilepsy) and is extended to include syndromes of mental deficiency, catatonia, paranoia, hysteria, psychasthenia, and manic depressive states, namadism, suicide, sedative addiction, crime. The section on neuropathology reviews the general data on neuropathology as evidenced by experimental extirpation and disease.

Part II deals with "Special Psychiatry," which follows closely a classification of disorders as determined by etiology. Consequently, although each disorder treated in this section is regarded as a special disease entity, differing in this respect from syndromes, there is a marked departure from the old hard and fast distinctions between various diseases. Thus, "so-called" schizophrenia is discussed in connection with chaotic sexuality, and "so-called" manic depressive psychoses become part of a more inclusive study of "affective-reaction" groups.

Part III deals with the practice of psychiatry—with methods of diagnosis and therapy, psychoanalysis, medico-legal questions, military and industrial psychiatry; and continues (Part IV) with a practical review of special diagnostic procedures: spinal fluid examination, Wassermann reaction, examinations for aphasia and mental deficiency, the Kent-Rosanoff association-test, and other special tests. Complete direction for the 1916 Stanford-Binet Test are included.

Part IV consists of one long chapter on "Mental Hygiene" dealing with eugenics, obstetrical problems, syphilis, prostitution, and other topics. A review is given of mental hygiene for all periods from infancy to maturity.

An appendix gives the official classification of mental disorders adopted in 1934, tables of height and weight norms, and tables of intelligence quotients. The author's causal or etiological classification of disorders has the effect of scattering the discussion of certain conditions having multiple causes. This apparent loss of system is offset by the gains made from practical and realistic points of view. The full index, in any case, will enable the reader to locate any topic with ease.

Wells College C. O. WEBER

*Varieties of Christian Experience.* By SVERRE NORBORG. Minneapolis, Augsburg Pub. House, 1937. Pp. 284.

The style here is so definitely forensic and hortatory one can easily imagine the author shouting forth his diatribes on psychology as he delivered the material in lecture form to a convention of Lutheran clergymen. The "studies of the average materialistic psychologists are" said to be "out-modish, by-gone and relatively unimportant." The "old psychology of religion is utterly biased and unscientific." The Bible is "the only truly realistic book in world literature." Depth psychology is all that can be accepted as scientific. Yet, in spite of such violent assertions, the author must not be condemned as an ignoramus. He appears to be a very erudite person,

especially familiar with Scandinavian literature. It is a pity that he is not as well informed concerning the work and aims of experimental psychology.

Readers familiar with the literature on the psychology of religion will be at once attracted by his thesis that Christian experience is distinctly different from mere religious experience. Through most of the volume the reader is lured by tantalizing hints that just a little farther along the author will prove this thesis, although it is perhaps with a touch of fear that he may be unable to do so. The fear turns out to be well grounded. The great distinction promised turns out eventually to be merely the contention that all Christian experience involves the hearing of the "Word of God." Certainly the hearing of quotations from the Bible in the course of a religious experience has not been overlooked by preceding authors as the present author would have us think, and as a feature of religious experience it most certainly does not make Christian experience psychologically unique. But, then, the reviewer is one of those psychologists condemned as hopelessly out-moded and unscientific. In his judgment, however, the book fails to make any contribution to the psychology of religion and is often misleading.

Indiana University EDMUND S. CONKLIN

*Schopferische Unvernunft? Rolle und Grenze des Irrationalen in der Wissenschaft.* By WILLY HELLPACH. Leipzig, Felix Meiner Verlag, 1937. Pp. 72.

Helmholtz's dictum that nothing is more practical than a good theory should be reversed, for a theory is good only when it is practically useful. The ideas which underlie theories are for the most part intuitive "hunches," the rational elaboration of which is an artifice acquiring significance only after the idea has been found to work empirically. Maxwell's differential equations of electricity would have remained a playful calculation proving nothing had not Hertz demonstrated the existence of electromagnetic waves, and the work of Hertz was only a laboratory exercise which awaited the practical applications of Marconi before the idea became truly significant.

A large measure of irrationality accompanies all scientific procedures and the value of a theory is limited by its possible application to the practices of doing and enduring.

The pragmatism advocated by the author is social and German. The "old English" variety of Oxford (Schiller?) and the "new English" model of Harvard (James?) are alike unworthy because they practice "prosperity" and hold it to be "humanity." The Germans, however, are blessed (or, if you will, damned) to seek, not the proximate but the remote reference, and to question of utility for what use, of service for what service, and of ends for what end.

Hellpach's monograph is an attempt to show that *Wahrheit* has no real meaning apart from *Wirklichkeit*, and that the irrational soul of a people is the arbiter both of creative thought and of the right to think.

Cornell University R. M. OGDEN

*Sex Life in Marriage.* By OLIVER M. BUTTERFIELD. New York, Emerson Books, 1937. Pp. 192; 4 plates.

Scientific and practical, with spiritual implications but no pious platitudes, this book grew out of the author's experience in various fields of marriage adjustment work. It aims to bring about not only a happy sex life but harmonious home living and environment for children.

Marriage patterns are traced historically through matriarchal and patriarchal marriage, in which authority and lineage stem from mother or father respectively; through polygamy and group marriage to the modern monogamous love marriage, which has developed through greater freedom and education of women. With separate homes and greater privacy, sex life has become a sharing of the whole personality.

Every couple should talk over, before marriage, factors concerning number and spacing of children, and agree on a mutually satisfactory plan of limitation, with a physician's advice and physical examination. If inhibiting factors emerge, such as shame, guilt, fear of pain, inferiority anxieties or fear of pregnancy, a counselor can usually discover and clarify the cause, which may be due to misguided or insufficient sex instruction or punishment for childish misdemeanors. The honeymoon should be passed in quiet and privacy. The chapter on techniques of intercourse is very detailed and practical, including mental attitudes, positions and procedures, timing and special adaptations and adjustments. Advice is given for prevention of nervous tensions by securing complete orgasm in women, prevention of premature ejaculation, equalization of desire and frequency, adaptation of intercourse to pregnancy, post-operation conditions and menopause, together with sexual adjustment in later life. Preparation for children and adaptation to them are discussed.

The chapter on sex anatomy and physiology is clear and concise, and accurate drawings and ample bibliographies add to the value of the work.

Psychologists in personal and adjustment clinics will do well to recommend this treatise both for its tone and its content.

Ann Arbor, Mich. BERENICE BARNES SHEPARD

*A First Course in Statistics*. By E. F. LINDQUIST. Boston, Houghton Mifflin, 1938. Pp. xiv, 226.

*Study Manual for A First Course in Statistics*. By E. F. LINDQUIST. Boston, Houghton Mifflin Co., 1938. Pp. 122.

The instructor who wishes to present the common statistical methods with an emphasis upon intelligent use and critical interpretation rather than upon the mechanics of calculation will find this text and manual admirably suited to his needs. The two books constitute an integrated course of study for the student of education or psychology in which the manual is an essential part of the whole. In working through the manual the student is led by a series of questions to work out *for himself* many of the characteristics and limitations of the measures defined in the text. Because of this active participation, and because of the care with which Lindquist has worked out his questions, statistical measures should become more concrete and meaningful to the student than they could be made by mere definition and dogmatic statement.

The textual treatment covers the common measures of central tendency, variability, sampling error, linear correlation, and the validity and reliability of tests. In each of these topics the treatment is fresh, and the methods are made reasonable without resort to mathematical derivations. This statement holds, surprisingly enough, even when we come to Lindquists's treatment of the Pearson product-moment formula for the coefficient of correlation. By presenting first the notion of standard error scores and allowing the student to become familiar with their characteristics, the product-moment coefficient is made a simple and logical next step. It is con-

sidered first in terms of standard error scores, and the raw score formula is presented only after this simpler treatment has been thoroughly discussed.

In brief, Lindquist has prepared a course in statistics from which the serious student should derive competence in dealing critically with test results and statistical interpretations in psychology or in education.

Cornell University T. A. RYAN

*Personality and the Cultural Pattern.* By JAMES S. PLANT. New York, The Commonwealth Fund, 1937. Pp. 432.

Sentences six, seven and even eight lines in length make this a very trying book indeed. The style is often turgid and at times even hypnotic. Yet the reader is induced to plow along because of the tantalizing implication that something profound, new, and of vast significance is just beyond the present paragraphs. The author is apparently seeking to present some new vision of social work and scientific method for the study of personality, although the reviewer finished the book with the conviction that this vision had not yet been clearly grasped by either himself or the author.

At the outset the reader is told that "the usual psychiatric formulations for the problems of children are inadequate" and then that out of several years of experience with the work of a juvenile clinic in Essex County, New Jersey, a new method has been developed which is superior to those of the psychologist, the sociologist, the anthropologist and the statistician for the study of personality. This new method is termed "the method of the casual breakdown" which appears after all to be no more than the study of the individual delinquent and of the various social pressures operating to bring about the delinquency. All summaries and generalizations are, however, eschewed as misleading. The chapters on the individual-centered culture, in which the relation of the family, the school, recreation programs, law and order, social work, medicine, the church, industry and education are presented in their relation to the developing personality, are fascinating because of the many brilliant ideas which spot their pages—but they are almost equally disappointing because of the dogmatism which pervades them. Many positive statements are made which if true would be most important additions to our knowledge. But are they true? Who knows? Authenticating references are far too often missing. All too frequently the reader is apparently expected to accept assertions on the basis of this new "method of the casual breakdown" in which systematic collection of evidence appears to be rejected. As an exercise in thought provocation and for the conception of problems for further systematic research these chapters can be heartily recommended to all psychologists and sociologists, but such persons must be willing to labor through the style in which they are presented. Fortunately there are periods in which the style clarifies. Whatever of lasting value there may be in this book must inevitably lie in its suggestions for research, probably by some of the very methods which the author rejects with such surprising assurance.

Indiana University EDMUND S. CONKLIN

*What Man Has Made of Man: A study of the consequences of Platonism and Positivism in Psychology.* By MORTIMER J. ADLER. Introduction by Franz Alexander. New York, Longmans, Green & Co., 1937. Pp. xix, 246.

The central core of this volume is made up of the notes which Dr. Adler used in giving a series of four lectures to the Institute of Psychoanalysis in Chicago. To

these are added an introduction by Dr. Alexander, an appendix of notes pertaining to the lectures and an epilogue. The number of pages devoted to the appendix and to the main topic of the book are nearly equal.

The four lectures cover the conception of science in the modern world, the place of psychology in this realm, its history and psychoanalysis as psychology. All of these are considered by one who is highly trained in and committed to the neo-scholastic point of view. As a result of this approach, Adler concludes that most psychology is not scientific psychology. In the first place, psychology can only study man as human, not as brute. Secondly, most psychology is a hodge-podge of psychology and poor (19th century) metaphysics. One of the worst doctrines which it has accepted from this philosophy is the theory of evolution. The only two branches of psychology which can hold any right to be called scientific are psychometrics (including psychophysics) and psychoanalysis. The final lecture is devoted to psychoanalysis as scientific psychology. After making the usual criticisms of the great amount of reification in psychoanalytic theory, Adler then points out how most of the principles which Freud has proposed as psychoanalytic theory can be found in Aristotle and St. Thomas. The chief difference between the two presentations is that Freud's precursors stated these principles in a much more acceptable form. Although the book is difficult to read, many psychologists who Dr. Adler says "are at once positivists and also philosophically doctrinaire" will be interested in an evaluation of their field by one who is not "philosophically doctrinaire" within the realm of the neo-scholastic school of philosophy.

University of Newark FREDERICK J. GAUDET

*Auto-Correctivism: The Psychology of Nervousness.* By V. E. FISHER. Caldwell, Idaho, The Caxton Printers, 1937. Pp. 337.

This book consists of twenty chapters, the first three of which are used to develop a background for the author's thesis. The chapter "Auto-Correctivism Defined" contains the nucleus of the book; namely, "that there exists some regulating principle which operates in such a manner as to thwart the overextension of either set of motives, thereby maintaining a quantitative balance between them." This principle, he feels, is "the common denominator which solves the mystery of the neurotic and his symptoms." He also states that "mental stability of an individual rests entirely upon a quantitative balance between the various pressures" exerted externally and internally by heredity, environment, and the individual's inherent capacity for development.

He carefully traces the development of the neurotic, tracing the stages through which an individual must pass to develop a neurosis. Five chapters are devoted to a detailed discussion of mechanisms. In ten chapters he describes various pathological conditions and analyses them, first enumerating and then describing each condition, its frequency, and illustrates it by giving selected excerpts from case history material, and then resolves the problem using his concept of the "auto-corrective principle" and its function.

This is a most difficult book to read, as the author has created an entirely new vocabulary which differs from any known definition that might be familiar to the reader. He disregards much of the approach to this problem provided by psychiatry or psychoanalysis. There seems to be a lack of knowledge of the most elemental motivation mechanisms of human behavior, as well as a failure to recognize that

abnormal behavior is a variation in degree from the normal. He is redundant and careless in the use of terminology which makes it increasingly difficult to follow. We see no excuse for the publication of this book.

Recorder's Court, Detroit, Mich. HELEN L. FLINN

*Résumé des Travaux Philosophiques et Scientifiques.* By BRANISLAV PETRONIEVICS. Bulletin de l'Academie des Lettres. Academie Royale Serbe, Belgrade, 1937. Pp. v, 328.

This book is a summary by Branislav Petronievics of his own philosophical and scientific works. It is divided into two principal sections: (1) a chronological listing of publications—there are 156 articles and books from 1896 to 1936; and (2) an abbreviated description of the author's contributions. The writings embrace such varied fields as philosophy, logic, mathematics, biology, paleontology, comparative anatomy, phylogeny, celestial mechanics, and the history of science. The chapter on psychology, 21 pages, is a summary of 17 articles and 6 books (on psychology or related topics). Some of the topics discussed are: depth perception, primary colors, elements of consciousness, and Fechner's law of psychophysics. A point of departure for much of the discussion is the work of such psychologists as Brentano, Ebbinghaus, Stumpf, Wundt, Hartmann, and Ziehen. The only American psychologist referred to at all is William James. It is surprising to note that no mention is made of the work of any living psychologist.

In his discussion, Petronievics analyzes and modifies many of the traditional psychological theories and concepts. His criticisms are, however, of doubtful value, since he neglects the tremendous accumulation of experimental data which has appeared since the original theories were proposed. There is no suggestion that Petronievics ever subjected his own theories to experimental test. The chapter on psychology, besides representing one of the many facets of productivity in the life of a remarkably prolific European scholar, is mostly of historical interest.

The Training School, Vineland, N.J. ANTHONY J. MITRANO

*Aspects of Science.* By TOBIAS DANTZIG. New York, Macmillan Co., 1937. Pp. 285.

This book is an analysis of the evolution of certain aspects of scientific thinking with especial reference to the increasing part that mathematics plays in such thinking. The author's analysis of the advantages and shortcomings of language as a sort of *rhetorical algebra* is particularly significant for those aspects of psychology which are not as yet reduced to mathematical statement. In his discussion of number, the author deals at some length with certain perceptual phenomena such as the two-point limen. He also presents a most interesting analysis of the relationship between the perceptual world and the world of geometry.

The treatment of the relationship between *models* and physical systems which have been developed is important. In summarizing the advances of the new physics, the author criticizes the "vague pronouncements" of certain physicists which can be interpreted as giving satisfaction to idealistic philosophy. Such pronouncements, the author says, are "meant mostly for popular consumption," for he concludes that the latest developments in physics may be interpreted as the final triumph of the mechanistic tendency.

The book contains an evaluation of the important work of Ernst Mach and thus

joins with the increasing chorus which recognizes in this physicist the herald of a new and less mystical age of science. It is not surprising, therefore, that the book ends with the words: "So let us hear the conclusion of the matter: Read your instruments, and obey mathematics; for this is the whole duty of the scientist."

Tufts College LEONARD CARMICHAEL

*The Wise Choice of Toys.* By ETHEL KAWIN. 2nd edition. Chicago, University of Chicago Press, 1938. Pp. ix, 154; 8 plates.

This book contains the lectures given by Miss Kawin during the educational exhibits presented by Marshall Field and Company in a coöperative plan with the Educational Department of the University of Chicago. The titles of the lectures capitalize the interest of adults in toys. Within this very tangible frame of reference the author has patterned a convincing presentation of growing personality in such a way that the reader clearly sees at work the factors of inheritance, environment and guidance, and experience as limiting and stimulating this growth. She utilizes scientific research in an informal and effective manner. This psychological aspect of play is especially well presented in portraying "growth in the child's interest in objective reality" from his "casual acceptance of the exterior world as it appears" to "his awareness of things as they are."

Toys are carefully classified by developmental age-levels—infancy, early, middle and later childhood and early adolescence; and within each age-level, by function—for development of strength and skill, for constructive and creative play, for dramatic and initiative play, for sound development, for artistic development, to stimulate knowledge, leading to interest in science and mechanics, hobbies and special interests. Toys are discussed for children with special needs, convalescence, particular ailments, and behavior and personality problems.

The practical suggestions in the book are organized in such a convincing pattern of psychological data that the reader sees new meaning and values in the play activities of all ages.

Cornell University ETHEL WARING

*The Psychology of Exceptional Children* (Vol. II). By NORMA V. SCHEIDEMANN. Boston, Houghton Mifflin Co., 1937. Pp. xii, 460; 4 plates.

The present volume is intended for use as a text, in courses given for teachers, upon the exceptional child. It provides an introductory account of a number of types of exceptional children not included in a previous volume by the author under the same title. After a chapter on the nervous system, successive chapters deal with the following types of child: the cerebrally birth-injured, the post-encephalitic, the enuretic, the albinistic, the pre-schizophrenic, the child afflicted with puberty praecox, the prematurely born, the bilingual, the eidetic, and the congenitally syphilitic. The various chapters follow a fairly uniform plan. Typical section headings are incidence, etiology, pathology prognosis, symptoms, sequelae, treatment, educational training. A glossary of technical terms is given at the end of the book.

As the section headings indicate, the book is organized on the basis of the concepts of clinical medicine. The title is something of a misnomer, since much of the material is not psychological in character. The style of the book is clear and direct, and the various chapters adequately documented. Recent research is well represented.

The book should be of value in meeting the needs envisaged by the author, as well as in providing a useful reference book for the clinical psychologist.

Cornell University GEORGE KREEZER

*Labyrinth und Umweg: Ein Kapitel aus der Tierpsychologie.* By J. A. BIERENS DE HAAN. Leiden, E. J. Brill, 1937. Pp. 231.

The aim of the author in writing this monograph is three-fold: (1) to present an historical and critical survey of the general problems and methods of maze and detour learning; (2) to summarize the results of experimentation principally with the lower vertebrates and the invertebrates; and (3) to arrive at a common explanation of maze and detour learning. The author's theoretical interpretations of maze and detour learning are based primarily upon studies with the rat. His criticisms of the several hypotheses of animal learning are current. The considerations that maze and detour learning differ only in degree lead to the conclusion that the learning processes can be classified only quantitatively. The "immediate grasping" of a new situation depends upon the utilization of past trial and error experience and not upon some "sudden" revelation. When the trial and error processes are progressively observable the author speaks of "gradual development" of spatial relations with regard to the goal. This does not, however, preclude that "immediate grasping" of a problem is devoid of the developmental process. The two classifications differ only with respect to the relative zero points in the different learning situations. In "trial and error" learning this point is arbitrarily selected, but this cannot be the case with "immediate grasping." The monograph includes an English summary and 250 references—most of them are American.

The reader will find this same material treated more comprehensively in the works of several American authors.

Carnegie Institute of Technology B. VON HALLER GILMER

*Emotional Hygiene: The Art of Understanding.* By CAMILLA M. ANDERSON. Cartoons by Dorothy G. Stevenson and Introduction by Ross McC. Chapman. Philadelphia, J. B. Lippincott Co., 1937. Pp. viii, 242.

Written by a physician who has had experience as a lecturer on topics concerned with mental hygiene and as an educator of nurses, the book becomes also a valuable aid to those who are seriously engaged in counselling and training of children and adolescents. It is divided into three parts: the biological and social bases of behavior, personality and adjustments, and emotions in relation to special fields, the last of which applies more directly to the problems connected with the nursing profession. Sample headings under the first of these three indicate the drift of the subject-matter: "Me First;" "We're Made That Way;" "Why Be Adult;" "Mind over Matter." The second and third sections include discussions of personal problems, patients, relatives, staff, the public health and school nurse, and the nurse as a witness.

The work has a delightful diction with only a minimal amount of illustrative case material necessary to point the facts. The attitude underlying the advice given is sound and is well indicated by the sub-title of the book. The cogent mental factors, usually of an emotional but sometimes also of an intellectual sort, which lead to maladjustments of the individual are well portrayed and the path to effective treatments is clearly and wholesomely outlined. In view of the many rather insipid

books on the subject, this presentation should be warmly welcomed, not only by the psychiatric and psychological profession, but by parents, clergymen, and social workers. It will be exceedingly helpful also to those who want to help themselves.

University of Iowa — CHRISTIAN A. RUCKMICK

*Child Guidance Procedures.* By PAUL L. SCHROEDER and others. New York, D. Appleton-Century Co., 1937. Pp. vi, 361.

Written coöperatively by members of the present staff of the Institute for Juvenile Research in Chicago, this book gives the history and philosophy of child guidance service as represented by the Institute from the early development under Dr. William Healey as psychopathic service for the juvenile court to its present state-wide service for Illinois families.

A member of each divisional staff, medical, psychiatric, psychological, social and recreational, has described the particular work of his division and the examinations, tests, and records of the diagnostic study. Apparently the different contributors had different readers in mind. The medical section, for example, is in more technical terms and is devoted almost exclusively to behavior dating from encephaletic diseases, whereas most of the sections are written for the general reader describing very simply psychological examinations and social work practices. For several representative cases the situation, diagnosis and treatment have been briefly described, illustrating the coöperative working of the staff on a given case.

The book is interesting reading for general information and explains in understandable terms the functions and values of clinical service. It is excellent to put into the hands of community leaders who may be in a position to make such service readily available in their communities.

Cornell University — ETHEL B. WARING

*Animals and Men: Studies in Comparative Psychology.* By DAVID KATZ. London, Longmans, Green & Co., 1937. Pp. xi, 263.

Katz's approach is that of psychological phenomenology. The book is, however, more than a summary of special researches. It deals essentially with problems; and, apart from the fact that it reports many hitherto unnoticed experiments, its greatest value to American animal psychologists lies in the wealth of stimulating problems which it formulates. After three general and methodological chapters, the author discusses in turn problems of perception, of motivation and of social behavior, and then concludes with a psychological comparison of men and animals. The book cannot be recommended as a textbook in animal psychology, since it makes no attempt to be comprehensive. It will, however, provide useful supplementary reading for such a course, and should be of considerable assistance both to the teacher and to the advanced student. Although Katz has drawn most heavily upon the results of his own experiments and of those of his pupils, he rounds out each discussion with a survey of the relevant material from other sources, and points his conclusions in the direction of further research.

Swarthmore College — R. B. MACLEOD

*To Drink or Not to Drink.* By CHAS. H. DURFEE. New York, Longmans, Green & Co., 1937. Pp. xi, 212.

The volume describes the methods used by the author in treating and often curing what he prefers to call the 'problem drinker' rather than the alcoholic. The thesis is

that the great mass of individuals who drink to excess do so as a flight from their difficulties or from an inferiority and that a cure can be obtained only by overcoming the basic cause. The methods make use of the more plausible psychoanalytic methods. Usually the patient must be taken from his usual environment for a time. For this Mr. Durfee uses a hospitable farm where physical work may be used as a distraction. Conversations discover and make clear to the patient the fundamental causes of his reliance upon drink. With understanding it is necessary to change habits that are connected with indulgence and to substitute for them new ways of life. The key note of the treatment is rebuilding the man not treating the symptoms of over indulgence.

The presentation of the thesis is very convincing. The results reported are very striking. The book is to be recommended for its sane approach to the whole problem and a new interpretation of many aspects of the alcohol problem incidental to the treatment of the victims. It can be recommended to the general reader as well as to the specialist.

University of Michigan W. B. PILLSBURY

*Die Schichten der Persönlichkeit.* By ERICH ROTHACKER. Leipzig, Barth, 1938. Pp. v, 107.

The book offers a theory developed after the pattern of Freud that would make a hierarchy of controls and awarenesses in the individual beginning with the lower reflexes, passing through the Freudian Id to the completely conscious voluntary controls. The Id lies at the same level as the emotions. It asserts that the mind is to be thought as made up of strata at different levels but still does not use the concept of development to explain the strata themselves. He speaks as if the higher were merely added to the lower without specific development.

The book offers little that is new. The most that can be said for it is that it finds some new analogies for representing rather obvious observations of the common man.

University of Michigan W. B. PILLSBURY

*The Body Taboo.* By ELTON RAYMOND SHAW. Washington, D.C., Shaw Publishing Co., 1937. Pp. 368.

While contrary to the impression given by its title, this book is neither professionally anthropological nor psychological, it may be of some value to those who are disposed to spend time on learning the arguments for nudism from a warm advocate. At least it will spare them the necessity of going native among the nudists, as Professor H. C. Warren was obliged to do in Germany, in order to grasp the philosophy and curative effect on social ills which are attributed to it. In the absence of opportunity for the flow of soul and feast of reason which the fellowship of nudism is claimed to induce, this book will provide almost equal entertainment. One feels that in the year 1937 its seriousness and crusading intensity are worthy of a somewhat more convincing cause.

Sarasota, Florida ELLIS FREEMAN

*Logic, Theoretical and Applied.* By D. LUTHER EVANS and WALTER S. GAMERTSFELDER. New York, Doubleday, Doran & Co., 1937. Pp. vii, 482.

This text is designed for undergraduate students and represents a successful and non-partisan attempt to include in one volume three typical approaches to logic:

(1) traditional formal logic, modified to meet the problems of modern science, (2) pragmatic logic with its emphasis on the psychological and sociological aspects of thought, and (3) symbolic logic. The space accorded to symbolic logic is brief. The book combines both clarity and depth of penetration, is richly supplied with exercises for students, and is well organized. A special feature of this text consists of special sections, occurring at appropriate points, devoted to amplifications of specific problems. These special sections are indicated in italics both where they occur and in the table of contents, and may be used as optional reading.

Wells College C. O. WEBER

*A General Selection from the Works of Sigmund Freud.* Edited by JOHN RICKMAN. London, The Hogarth Press & Institute of Psychoanalysis, 1937. Pp. xi, 329.

As the title implies this is a reprint of many of the shorter works of Freud. It opens with the lectures given at Clark in 1910 and ends with "The Inhibitions, Symptoms and Anxiety" of 1926. Some of the better known titles are: On Narcissism, Beyond the Pleasure Principle, the Ego and the Id.

Altogether the selections are typical and give a good idea of the applications of the Freudian doctrines to the abnormal processes. A bibliography and glossary increase the usefulness of the book.

University of Michigan W. B. PILLSBURY

*A Mind Mislaid.* By HENRY COLLINS BROWN. New York, E. P. Dutton & Co., 1937. Pp. 219.

The psychologist may well be absorbed by this rambling account of the author's years in Bloomingdale, recovering from what he describes only as a 'nervous breakdown.' He may be irritated by its tone of heavy irony, by the lack of technical editorial supervision, and by the variation in style from the forced levity used in describing 'cuckoo land' to the evangelistic sonority of the later chapters. He may chuckle over the 'arterial veins' from which blood samples were taken, or over the author's reading in "Behaviorism, Mother Love, Alcoholism . . ." or over the sad case of the boy who suffered from Behaviorism. In any event, he will classify the volume with a number of similar books which have appeared recently and will not be tempted to put it on the same shelf with "A Mind that Found Itself."

University of Maryland JOHN G. JENKINS

*The Amateur Actor.* By FRANCIS MACKENZIE. 2nd edition. London, Thomas Nelson and Sons, 1936. Pp. 172.

This is a new edition of a little book which first appeared in 1935. The author is the Organizing Director of Drama Schools in the British Drama League, and the book is published in Nelson's "Little Theatre" Series. In plain and clear language the reader is first told many things that the amateur actor should know. Then, exercises are given, every one of which is designed to develop a particular point, such as timing in making an exit, change of mood, and improvisation. The psychology involved in such discussions as "effect of emotion on movement" and "gesture" is sound.

Cornell University H. P. WELD

*Psychologische Diagnostik.* By RICHARD MEILI. München, Ernst Reinhardt, 1937. Pp. 251.

An excellently conceived and written book, which might in the United States go under the title "Educational Psychology" except that it does not treat educational methods of teaching and administration in general but restricts itself to the diagnosis of individual needs. It differs from many other books published in Germany by the fact that sources in the English and French languages are used as widely as German ones and are made available to the reader. It originated in the Institut J. J. Rousseau in Geneva.

Coral Gables, Fla. MAX F. MEYER

*Visualizing the Curriculum.* By C. F. HOBAN, C. F. HOBAN, JR., and E. B. ZISMAN. New York, Cordon, 1937. Pp. 300.

Psychologists interested in methods of teaching their subject will find parts of this book very stimulating. Although written primarily with the problems of high-school and elementary teachers in mind, it offers a challenge to higher education as well. Of all groups, college instructors have been the most backward in applying the principles of effective visualization and in adapting the newer types of visual aids to their needs. Reading the authors' persuasive arguments should work toward elimination of this lag. Psychologists will be especially interested in Chapter I, dealing with visual aids and the psychology of learning, and in Chapters IV and V, dealing with the amateur production of film and slide materials. *Visualizing the Curriculum* might be retitled *Vitalizing the Curriculum;* the attractive format of the book carries out its precept.

Northwestern University G. L. FREEMAN

*Die Prophetie.* By ABRAHAM HESCHEL. Krakow, Publication of the Polish Academy of Science, 1936. Pp. vi, 194.

This is an extensively documented monograph which provides a novel interpretation of the pre-exile Hebrew prophets. No attempt is made by the author to explain the phenomenon of prophecy or to evaluate it. The distinctive contribution of this work is the analysis of the constituent elements of the prophetic act and the inner experiences of the prophet. His conclusion is that the prophecy of such men as Amos, Jeremaiah, and Isaiah, represents a phenomenon *sui generis* differing particularly from religious ectasy, oriental manticism, the Greek oracle and other similar phenomena among pre-literate people and in ancient civilization.

Columbia University THEODORE ABEL

*The Treatment of Moral and Emotional Disturbances.* By CYRIL VALENTINE. New York, Macmillan Co., 1938. Pp. xi, 148.

This little book seems well adapted to its purpose, that of being a practical guide to parsons, social workers, family physicians, and others, who may be called upon to deal with minor cases of neurosis and psychic disturbance. It is neither a systematic introduction nor an advanced text book, but occupies a middle ground. It presupposes some knowledge of psychology and psychiatry and some experience with, or at least interest in, cases of moral and emotional disturbance, but does not enter into long or theoretical discussions of these subjects. It has two Introductions, one

by the Rev. Harold Anson, Master of the Temple; and another by Dr. J. R. Rees, Director of Tavistock Clinic, London. Some of the chapter headings are: "Psychological Surgery," "The Need for Minor Operations," "The Horrors of Psychological Standardization," "Helping People with Moral and Emotional Difficulties," and "The Readjustments of Personal Relationships." Dr. Valentine's little book is marked by moderation and common sense, and would seem to be a good book for anyone who had occasion to deal with miner cases of mental disturbance to have in his office.

Hobart College — FOSTER P. BOSWELL

*Il Concetto Della Indeterminazione.* By LEONE VIVANTE, Firenze, Vellechi Editore, 1938. Pp. 281.

According to the author indeterminism, which is the same thing or process frequently referred to as liberty, actual cause, significant cause, etc., but not fortuity, is a genuine psychic occurrence in the field of spiritual becoming or productivity. Vivante thus aligns himself with Bergson, Croce, and Gentile in espousing a creativity metaphysics of objective Spirit. With such a background indeterminism, final cause, and freedom can be derived, of course, without any difficulty.

Vivante's book, however, is divided into two parts, both of which he appears to think constitute potent efforts to establish indeterminism. In the first the author considers and answers a long list of objections to the concept of freedom, while the second is devoted to the definition and exposition of indeterminism and a list of arguments to demonstrate it.

Because of the metaphysical basis and background indicated it is only to be expected that those accustomed to scientific ways of thinking and to conclusions drawn from observed events can hardly fail to be determinedly unsympathetic with the problem of this work and the methods by which it is solved.

Perhaps the only value the psychologist can find in such a book is its illustration of a particular form of intellectual activity.

Indiana University — J. R. KANTOR

*Naturalism in American Education.* By GEOFFREY O'CONNELL. New York, Benziger Brothers, 1938. Pp. xxvi, 285.

This book, an amplification of a doctoral dissertation, castigates the American educational system, particularly that phase of it which is dominated by the philosophy of Dewey, and the application of that philosophy to educational practice by Rugg, Kilpatrick and Thorndike. A sketchy survey of the development of naturalism is offered as a background of current educational practice, and to it are ascribed the ills of modern youth. As a substitute for such a system O'Connell advocates the adoption of the type of education which will train the will, discipline the self, and under the guidance and with the aid of religion will stabilize and develop the individual's mental and emotional life "so that his intellect may attain truth and observe order."

Mount Holyoke College — HERBERT MOORE

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